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PART I.
ORIGINAL COMMUNICATIONS.

ART. XI.—*On Paralysis occurring during Gestation and in Childbed.* By FLEETWOOD CHURCHILL, M.D.T.C.D., M.R.I.A., Fellow of the King and Queen's College of Physicians in Ireland, and Associate Member of the College of Physicians, Philadelphia, U. S., &c. &c.^a

HAVING been much interested by a case of paralysis after delivery, to which I was called by my friend Dr. Duke, I was led to make some inquiry into the occurrence of the disease, not merely after delivery, but during gestation, and to examine the authorities within my reach, in order to collect from them all the information they contain; but I am sorry to say that my search has not been very fruitful in results. Bearing in mind that paralytic affections may occur as a termination of convulsions, as well as independently, I carefully looked over the principal obstetric writers; and in the works of Giffard, Ould, Exton, Smellie, Chapman, Pugh, Burton, Moore, Foster, Perfect, Osborn, Spence, Hamilton, Denman, Burns, Merriman, Blundell, Collins, Davis, Lee, Murphy, and Hardy and M'Clintock, I have found no mention of the disease.

^a Read at a meeting of the Association of the College of Physicians, March 1, 1854.

Dr. Campbell has a short section on paralysis of the pelvic extremities, which, he observes, "may be either partial or complete; and in all the cases which I have seen is confined to one of the limbs. It must be owing to the long duration of the head in the pelvis, from disproportion and consequent injury to the pyriformes muscles, or great sacro-sciatic nerves. The partial variety is what we generally meet with, and in either, it is a protracted complaint, without the patient, however, in any instance that I have seen, becoming permanently lame"^a. Dr. Ryan observes, that "some women, after the easiest as well as after instrumental delivery, are attacked with paralysis of the lower extremities, which is generally attended with retention of urine. The disease usually disappears in a few weeks"^b.

Dr. F. Ramsbotham states that "paralysis of one or both legs, in very various degrees, occasionally happens after labour; more frequently when the process has been tedious and painful; but sometimes, when it has been of ordinary duration, or even of unusual rapidity. It is not attended with cerebral affection, and is dependent on the pressure which the muscles and nerves have sustained during the passage of the child's head through the pelvis. There is pain or numbness both within that cavity and around the hip, and an inability to move the limb with freedom. It generally disappears by degrees within a few days; at other times it continues beyond the period the patient usually remains in bed, and compels her, when she rises from it, to use a stick or a crutch." Again, "hemiplegia, indeed, may appear after delivery, as well as at other times, but there will be particular symptoms, independently of the local affection, which are too well known to require mention from me here"^c.

Dr. Dewees has given two cases of convulsions, followed by temporary blindness^d; but this is the only reference to the subject in his works, or those of Dr. Meigs.

I have not been more fortunate in my search among systematic writers on diseases of women, for neither Leake, Hamilton, Blundell, Hall, nor Ashwell, makes mention of either paraplegia or hemiplegia.

I find as little notice of these affections in French or German obstetric works. I have examined the writings of Saccombe, Boivin, La Chappelle, Baudelocque, Maygrier, Gardien, Velpeau, Moreau, Chailly, and Jacquemier, of Carus, Jöerg, Wigand, and Busch, on midwifery; and of Nauche, Capuron, Gardien, Jöerg, and Siebold, on diseases of women, without hav-

^a Midwifery, p. 406.

^b Manual of Midwifery, p. 661.

^c Obstetric Medicine and Surgery, p. 548.

^d Midwifery, p. 506.

ing been able to find an allusion to it. The recent work of M. Scanzoni, of Wurzburg, is the only one in which it is noticed. He has a chapter on paralysis of the lower extremities, in which, admitting that it may in some cases be owing to pressure, yet as it may not appear until some time after labour, and as a similar affection may attack the upper extremity, he considers that pressure cannot be the sole cause, but that it may be attributed to some more profound derangement^a. He has given a case which I shall quote hereafter.

This paucity of information in systematic obstetric works, it occurred to me, might be owing, not solely to the rarity of the disease, but partly to the opinion that the disease belonged more properly to the department of general medicine, and that, perhaps, I should find more information in works treating of the practice of physic generally, or of diseases of the nervous system in particular. I have, therefore, consulted such as are within my reach, but with very limited results. The disease, as connected with pregnancy or childbed, is not mentioned by Hasse, Rokitanski, or Abercrombie, but the latter distinguished observer has some observations so apposite to the cases which I shall relate presently, that I shall take the liberty of quoting them. He states, that an attack of paralysis may^b—"1. Be merely the prelude to the apoplectic, and may pass into it after a short interval. 2. The attack may, under proper treatment, pass off speedily and entirely, leaving, after a very short time, no trace of its existence. 3. The recovery may be very gradual, the use of the affected limbs being restored after several weeks or months. 4. The palsy may be permanent, &c. &c." And again, he remarks, that "the whole phenomena of palsy do indeed bear evidence that certain cases of it depend upon a cause which is of a temporary nature, and capable of being very speedily and entirely removed. We see hemiplegia take place in the highest degree, and yet very rapidly disappear; but the most singular circumstance connected with certain cases of palsy is, that we occasionally see it continue without any improvement for many weeks or months, and then, from some change which entirely eludes our observation, take a turn for the better, and entirely disappear." Dr. Todd^c mentions the fact, that anæsthesia of the face sometimes occurs after parturition, and also that paralysis may occur, and that it is sometimes coincident with phlegmasia dolens.

Dr. Cooke, in his work on Nervous Diseases, does not treat

^a Lehrbuch der Geburtshülfe, p. 1000. ^b Diseases of the Brain, &c., pp. 246, 248.

^c Cyclopædia of Practical Medicine, vol. iii. p. 243.

of paralysis in connexion with pregnancy or parturition, but he notices a curious fact, which is illustrated by one of the cases I shall detail, namely, that a patient with hemiplegia is sometimes unable to utter the exact word they wish, to express their meaning, either from forgetting it, or from a difficulty in pronouncing it^a.

Dr. Graves, in his admirable observations on the Pathology of Nervous Diseases, when treating of the centric or excentric origin of certain forms of paralysis, remarks: "I shall endeavour to prove, first, that paralysis (from whatever cause) affecting one portion of the circumferential extremities of the nerves, may also affect other portions of their extremities; secondly, that pain originating in one situation may produce a similar sensation in distant parts; and, thirdly, that convulsions, resulting from irritation in any part of the extremities of the nervous system, may occasion a corresponding train of symptoms in other parts of the body"^b. Although I think that these observations bear directly upon the affection under consideration, they were not so associated in Dr. Graves' mind, for he makes no allusion to paralysis occurring before, during, or after labour.

In Dr. Watson's Practice of Physic there is a very full and able account of the different varieties of palsy, but no mention of its occurrence in pregnancy or childbed; nor yet in the more recent special treatise of Dr. Copland.

I may further observe, that in none of these works is there any notice of the condition of the urine previous to or immediately after the attack. The only author who up to this period seems to have suspected a connexion between serious nervous affections and certain states of this secretion is Dr. Latham, who, after enumerating various diseases attended with or caused by albumen in the urine, says of the brain, "And some of its graver affections will come and go, and admit of a present relief, which is unusual where harm has befallen its own structure: convulsions and apoplexies appear and disappear, and yet are ultimately fatal, the chief concomitant circumstance which attracts our notice being albuminous urine"^c.

Dr. Romberg, in his valuable work recently published by the Sydenham Society, has a section upon "paralysis depending upon the affections of the sexual organs"^d, which is of such interest that I trust a pretty long quotation will be excused. "The female sex," he observes, "offers peculiar opportunities

^a Vol. ii. p. 10.

^b Clinical Medicine, by Neligan, vol. i. p. 501.

^c Diseases of the Heart, vol. ii. p. 229.

^d Diseases of the Nervous System, vol. ii. p. 390.

for the study of paralytic attacks connected with morbid conditions of the sexual system: they arise either from direct pressure of the distended uterus, or ovary, upon the nervous plexuses of the lower extremity, and are then only unilateral, and accompanied by derangement of sensibility, as pain, numbness, or loss of sensation; or they are caused by a reflex influence upon the spinal cord, and then affect both sides of the body. Veterinary surgeons have repeatedly met with the complication of paraplegia and metritis; Gelle^a quotes eleven cases of acute metritis in cows, which had followed calving; in all, the power of moving the hind legs was diminished, while sensibility continued unimpaired. Sewell^b publishes the *sectio cadaveris* of a cow attacked with paraplegia following calving; intense inflammation was found in the uterus and vagina. Ithen^c has communicated a few cases of metritis in mares, which was accompanied by inability to stand, and to raise themselves on their hind legs. Dr. Hunt^d has observed similar occurrences in women. Lisfranc^e details the case of a lady, aged 36, who had gradually been attacked with paraplegia, without any loss of sensibility. All the remedies applied on the assumption of a disease of the spinal cord remained unavailing. Lisfranc made a vaginal examination, and found the fundus uteri so much enlarged as almost to fill the pelvis; after using the iodide of potassium and iodine frictions for three months, and taking the waters at Barège, the tumefaction of the uterus was reduced, and complete recovery was obtained in two years.

“Paralysis of the lower extremities often supervenes during confinement and even after easy births, without pain having preceded or being associated with it; and the accoucheurs attribute it to compression of the sciatic plexus and obturator nerve, for which there is no warrant whatever. There would be more justice in assuming that an accumulation of serous fluid had taken place in the spinal canal, resulting from disturbance of the circulation, as we find in tumours of the abdominal cavity; but morbid anatomy does not afford any proofs of the fact. No such condition, however, is necessary, as demonstrated by a case which recently came to my notice, and in which the uterus was atrophied. A woman, aged 41, had, since the cessation of her catamenia six years previously, frequently suffered from twitching of the inferior extremities,

^a Journal Pratique, &c., 1826.

^b Veterinarian, vol. iv. p. 509.

^c Nebel und Vix. Zeitschrift für die Gesam. Thierheilkunde, vol. iii.

^d Stanley, p. 274.

^e Journal de l'Anat. de la Physiol. et de la Pathol. du Système Nerveux, 1843, vol. i. p. 154.

and for nine months past, motility had been diminished; there was a frequent tremor and sense of weight in the legs. The cutaneous sense of touch was dull in the feet, especially in the left, in which the disease had commenced with lacerating pains. Retention of urine, alternated with enuresis, especially at night. The upper extremities continued in the full possession of their vigour. No abnormality was discoverable in the spinal cord. Dr. Schöller, whose accurate method of exploration is well known, found that the vagina was much shortened, and that there was no portio vaginalis, only the transverse fissure of the uterus remained visible at the arch of the vagina, directed from before backward. The os tinæ was very soft, and the neck presented the usual hardness; it was evident that the sexual system had undergone a process of involution, as in an old woman. I ordered the alkaloid of the *nux vomica*, strychnine, in doses of a quarter of a grain, twice a day, and friction of the legs with the ethereal oil of turpentine; after continuing this treatment for three weeks, a favourable effect was experienced."

A friend very kindly undertook a tolerably extensive search among the periodicals, but, with two remarkable exceptions, nothing on the subject was found. These two exceptions were, a paper by Dr. Lever, of London, and one by Professor Simpson, of Edinburgh. With both of these gentlemen I have been in communication, and from both I have received additional matter, a favour whose value has been doubled by their frank, kind manner of conferring it, and for which I take this opportunity of returning my warmest thanks.

Dr. Lever's cases of paralysis form part of an interesting series, illustrative of certain nervous affections of pregnancy; and in support of his conclusion that "pregnancy is occasionally associated with chorea, or convulsive movements; with paralysis of various parts of the body, of the extremities, and of the nerves of special sense; and with mania"^a. The varieties of paralysis he describes are, amaurosis, deafness, hemiplegia, loss of power in both upper extremities, loss of power in the right lower extremity, &c. &c. I shall take the liberty of giving an abstract of these cases by and by.

Dr. Simpson's communication, published in the same year, consists of some observations made at the Edinburgh Obstetrical Society, from which I shall extract so much as relates to the subject before us at present. "1. Albuminuria, when present during the last periods of pregnancy and labour, denotes a great and marked tendency to puerperal convulsion. 2. Al-

^a Guy's Hospital Reports, 1847, vol. v. p. 1.

buminuria in the pregnant and puerperal state sometimes gives rise to other and more anomalous derangements of the nervous system, without proceeding to convulsions; and Dr. Simpson had, especially, observed states of local paralysis and neuralgia in the extremities, functional lesions of sight (amaurosis, &c.) and hearing; hemiplegia and paraplegia more or less fully developed. . . . ” “ 5. Albuminuria and its effects are far more common in first than in later labours, and then constitute a disease which generally disappears entirely after delivery. But Dr. Simpson had seen one case commencing with slight blindness, but no œdema, and ending gradually in hemiplegia, where the palsy remained after delivery, and after the disappearance of the albuminuria. In another, amaurosis came on with delivery, and had been present for six months, when Dr. Simpson first saw her. She had no œdema or other symptom of albuminuria, but in testing the urine, it was highly albuminous. 6. Albuminuria with convulsions, &c. occurring in any labour later than the first, generally results from fixed granular disease of the kidney, and does not disappear after delivery. 7. Perhaps, in puerperal convulsions, &c., produced by albuminuria, the immediate pathological cause of the nervous lesion is some unascertained but poisoned state of the blood. Was there a morbid quantity of urea in the blood? In several specimens of the blood of patients suffering under puerperal convulsions, furnished by Dr. Simpson to Dr. Christison and Dr. Douglas Maclagan, these gentlemen had been unable to detect any traces of urea. Was the poisoning material caseine in morbid quantity or quality? The dependence shown by Gluge and others, of albuminuria upon steatorrhea of the kidney, makes this connexion worthy perhaps of some inquiry.” “ 9. Sometimes hemiplegia supervened during pregnancy, without albuminuria, but this form did not seem to interfere materially, or very dangerously, either with the pregnancy or labour; the disease running its own usual course. In one case, Dr. Simpson had seen the patient gradually but imperfectly recover the use of the palsied arm after delivery. In another, no improvement occurred”^a.

Let us now see to what the information we have obtained from these different authorities amounts. Very briefly, we find:—

1. That hemiplegia, paraplegia, or partial paralysis, may occur previous to, during, or some time after labour.

2. That by some authors, the paralysis, in paraplegia especially, is attributed to pressure upon the muscles or nerves, in

^a Edinburgh Monthly Journal, October, 1847, p. 288.

prolonged labour; but this is also denied, as the same disease follows easy labour, or occurs after the lapse of some days.

3. Paralysis may terminate convulsions or accompany them.

4. Paralysis may be the consequence of organic disease, or of effusion into or upon the brain or spinal marrow.

5. Paralysis may result from reflex action.

6. The palsy may depend upon temporary causes, and among such causes albuminuria may be included.

7. Hemiplegia may run on into apoplexy, or it may pass off in a few weeks, or sometimes more slowly. Paraplegia may leave a temporary or more permanent lameness: the local palsies (amaurosis, deafness, &c.,) generally last but a moderate time.

8. A nervous or hysterical paralysis may occur occasionally in the unimpregnated state, or during pregnancy, but that it seldom continues after delivery.

I shall now proceed to give a short abstract of such cases as I have been able to collect from authorities, or from my professional friends, or which I have seen myself. Among the former, I am indebted to Dewees, Scanzoni, Crosse, Beatty, Lever, and Simpson; and, among the latter, to Drs. Lever, Simpson, McClinton, Beatty, Forrest, Ireland, and Duke.

I shall first give those in which the paralysis occurred during pregnancy; then those in which it occurred at the time of labour; and lastly, those in which it followed parturition.

I.—PARALYSIS OCCURRING DURING PREGNANCY.

CASE I.^a—*Hemiplegia, partial.*—Mrs. A., aged 26, has two children; was attacked with “numbness in her right arm, and a diminution of its power. The mouth is drawn slightly to the right side, and there is a feeling of occasional tingling, and sometimes numbness, in the left thigh, leg, and foot. This was when she was about two months pregnant. Under careful treatment these symptoms diminished, but she did not lose them until her confinement. Similar symptoms accompanied the next pregnancies. Suffice it to say, that I have now attended this lady in four pregnancies, and with four children, she having borne six; that the same symptoms make their appearance very soon after pregnancy takes place; that they become modified by treatment, but are never removed until after delivery; that the period of their cessation has seemed to depend upon the nature and amount of blood lost during labour, &c., &c.”

CASE II.—*Paralysis of Right Arm and Hand.*—S. M.,

^a Dr. Lever, Guy's Hospital Reports, vol. v. p. 12.

aged 38, married for nine years, had miscarried on two occasions, and during the eighth month of pregnancy was attacked by a "tingling sensation in the palm of her right hand and fingers, which rapidly ran up the extremity to the shoulder and axilla, when she found herself unable to hold or feel her needle; the fingers were slightly flexed on the hand, the hand on the fore-arm, and the fore-arm on the uterus." Five days after the attack Dr. Lever saw her. "The right extremity was in the condition above described; the sensation of the limb appeared little, if at all, deranged; but she complained of a sensation of heat throughout its whole extent: her countenance was pale; there was no pain or heat in the head; her bowels had been freely opened on the day of my visit; her pulse was small, feeble, and 96"^a. Under the use of sulphate of zinc, with nutritious diet, &c., she improved, but did not recover the use of her arms until after her confinement.

CASE III.—*Paraplegia*.—"Is that of Eliza H., who was in Guy's hospital several times (I believe three) under the care of my late colleague, Dr. Ashwell. Immediately after her sixth labour, which was perfectly natural, she felt a great numbness in her lower extremities, as well as weakness, but from these she gradually recovered. During her seventh pregnancy, about the third month, her lower extremities gradually became paralyzed, and this time she was unable to stand or walk, and was compelled to keep her bed. After her confinement she was carried into the hospital, and placed under Dr. Ashwell's care, when she perfectly recovered. On going out, she became pregnant with her eighth child, and paralysis did not come on until after labour"^b.

CASE IV.—*Amaurosis*.—Mrs. T., aged 31, soon after quickening of her fifth child, "whilst engaged in some plain needlework, suddenly felt a peculiar sensation in the eye-balls, and found, on opening the lids, that she could merely see the outline of objects, their centre being perfectly dark." "Her eyes were dark; the pupils were large, and contracted (though sluggishly) upon the stimulus of light; the eye-balls seemed to have in a measure lost their mobility, and to be inordinately fixed; the eyelids altogether, or nearly, covered the globes, for if they were not protected, she complained of a sensation of dryness and smarting." This state continued until her confinement; in a week afterwards there was an improvement which increased until after her return from the country: at the end of three or four months she could see as well as ever^c.

^a Dr. Lever, Guy's Hospital Reports, vol. v. p. 14. ^b Ibid. p. 16. ^c Ibid. p. 17.

CASE V.—*Deafness*.—Mrs. S., aged 23, when about three or four months pregnant of her first child, noticed that her hearing was not so acute as before; and it was evident that, week by week, it was becoming more obtuse. “She was dispirited and pale; her tongue, when protruded, was tremulous, flabby, and indented; her pulse small and feeble; her appetite tolerably good; her bowels regular; her nights restless; and the irritability of her temper had increased.” The deafness continued to increase, in spite of treatment, until after her labour, which was natural. “The day after, she said her hearing was better, so that by the time she went to church, she could hear as well as ever”^a.

CASE VI.—*Hemiplegia*.—Mrs. P., aged 18½, in the seventh month of her first pregnancy, fainted, in consequence of some family disputes, and when she recovered “it was found that she was hemiplegic on the right side, as far as the upper part of the abdomen; no pinching, tickling, or any other irritation, caused any movement in the right lower extremity. On the following day she swooned again, and this was followed by an apparent loss of motion and sensation in the right upper extremity. Matters continued the same for three days, when the limbs suddenly regained their usual power. Speechlessness now ensued, which also lasted for three days, and was followed by loss of power in the right lower extremity as far as the knee.” The symptoms improved, but continued until she was delivered. In a subsequent pregnancy she suffered from speechlessness for a fortnight, and after delivery she found she had lost all power of the lower extremities, but this she gradually regained^b.

CASES VII., VIII.—*Hemiplegia*.—In addition to the preceding cases, which I have condensed from his paper, Dr. Lever, in a letter which I have his permission to publish, writes: “I have since seen two cases of hemiplegia depending upon cerebral disease, in which gestation proceeded to the full time, labour progressed, and the patient recovered. The child of one was hydrocephalic.”

CASE IX.—*Facial Paralysis, Partial*.—“I know of one woman who had had two deliveries and is now pregnant. She counts her pregnancy by finding numbness and want of power on the right side of the face, with a sensation of ‘pins and needles’ in her right hand. She recovers after delivery.”

CASE X.—“Another lady has had eight children, and has always suffered in a similar way. She has just aborted. Neither of these patients could nurse their infants.”

^a Dr. Lever, Guy’s Hospital Reports, vol. v. p. 18.

^b Ibid. p. 20.

CASE XI.—*Amaurosis*.—"In the course of this year (1853) I was requested to see a lady about thirty years of age, highly sensitive, nearly eight months pregnant, who was suffering from amaurosis in one eye, perfect, in the other the loss of vision was not so complete. She had had two fits, but not having the opportunity of meeting the medical attendant at my first visit, I did not learn their nature; however, by her appearance and her evidence, I was induced to suspect the presence of albumen in the urine. The medical man applied the usual tests, and found that this was the case; and as the vision became more and more impaired, the operation for the induction of premature labour was resorted to. Labour pains commenced twenty-four hours after the rupture of the bag of the waters, and in twelve hours the child was expelled, still-born. Three weeks since, her husband told me that his wife could play a game of cribbage."

CASES XII., XIII.—*Deafness*.—"In two instances I have known the sense of hearing, sorely blunted during pregnancy, restored after confinement; but albumen was not to be detected after delivery. When the albumen has been found wanting, the acuteness of hearing has returned." Let me add, that Dr. Lever mentions that in every case of this kind in which he examined the urine, he found it albuminous.

My friend, Professor Simpson, has kindly furnished me with the following notes of cases which have come under his observation:—

CASE XIV.—*Hemiplegia*.—Mrs. —, the daughter of a distinguished physician, when nearly eight months pregnant, became slightly amaurotic (palsy of the fifth): this led Dr. Simpson to examine the urine, which he found coagulable. Before labour came on, hemiplegia gradually supervened. The patient recovered, to a certain extent, after her confinement; she is now able to walk about, but has not yet the complete use of the affected side. In a subsequent letter he mentions another case of great interest.

CASE XV.—*Facial Paralysis*.—"The lady is within a week or two of her third accouchement. Four days ago, after feeling unwell, she felt a stiffness in the right side of her face, preceded by pain in the back of the head. The stiffness soon assumed all the usual symptoms of paralysis of the portio dura. When I saw her to-day, the face was much disfigured, particularly when smiling; she could not close the right eye, &c.; and, in addition, there appeared a want of sensation about the cheek, nose, and lips, as if the sensitive branches of the fifth were also affected. The eyelids, but especially the right, were swollen and œdematous, but there was no œdema of the hands or feet,

or elsewhere. The pulse was very slow and weak. She was super-sensitive to sounds, light, &c.: but the point that will interest you is this, that on testing the urine, I found it become very opaque and thick, on boiling." "I have seen a number of instances of local paralysis, particularly of the eyes, in connexion with albuminuria, but not until the present case, any example of paralysis of the seventh pair."

CASE XVI.—*Hemiplegia*.—He adds, "Since writing to you, I have seen a patient who became hemiplegic six years ago, with her first child, and who has only imperfectly recovered the use of the affected side. From the symptoms accompanying the attack, it was probably another instance of this result from puerperal albuminuria."

The following cases occurred in the practice of Dr. Crosse of Norwich:—

CASE XVII.—*Hemiplegia*.—"Mrs. ——— was delivered of twins in May, 1844; a feeble, slender woman; had paralysis of the left side of the face before she married, which always remained; also the right became paralyzed after her labour, under a reducing diarrhœa, but this was recovered from. In the course of this year (1846) her paralysis of left side increased; she emaciated greatly; during these unfavourable changes there were sickness and indications of pregnancy. In June she had become so feeble as to take to her bed-room, and after some weeks could scarcely get out of her bed to have it adjusted. She passed her water only once in twenty-four hours, and at length had great difficulty in speaking and in swallowing,—all signs of her increasing paralysis or increasing disease of the brain." "She sunk in powers and bulk as pregnancy advanced; at the end of September could swallow only liquids and was much troubled with the mucus, which she could neither swallow nor expel by the mouth; very threatening paroxysms of suffocation were produced by this. In the beginning of October, she was evidently sinking fast, relieving us from the fear of delivery at full time, which she could not have survived"^a. She died October 12, but no post-mortem examination was made.

CASE XVIII.—*Hemiplegia*.—"Mrs. P., aged 42, mother of several children, six years ago had a slight paralytic stroke when pregnant, but went on to the full term of utero-gestation and recovered. Complained of numbness of right leg and arm some days before, and became quite hemiplegic of that side at midnight, September 2, 1827. Bleeding, blistering, and open-

^a Cases in Midwifery, &c., by J. J. Crosse, M.D., F.R.S., p. 162.

ing medicine, employed. The liquor amnii began to dribble a few hours afterwards, and she was observed to strain as if in labour now and then, though she said she had no pain. In twenty-four hours a child between six and seven months was born dead, and the placenta followed. Although she strained, she did not appear to have the usual severe pain from the action of the uterus, and only called out at the last few pains when the child was passing through the os externum. Sept. 5th. She remains hemiplegic, but in all other respects seems doing well"^a.

CASE XIX.—*Hemiplegia*.—Mrs. B., aged 27, was seized with hemiplegia a month before delivery, she recovered considerably before labour came on, on the 17th of May. "On the 9th of October she had slowly recovered almost the entire use of the limbs, but is thin, and feeble in mind as well as body." In 1845 she was again confined without any paralytic symptoms before or after labour^b.

I am indebted to my friend Dr. Beatty for the two following cases:—

CASE XX.—*Facial Paralysis*.—Oct. 1st, 1850. Mrs. —, first pregnancy. This young lady expected her confinement about the end of this month, and was very much shocked, on going to dress herself this morning, to find her face crooked; she had no notice of the paralysis that seized the muscles of her face, and her first knowledge of it proceeded from seeing herself distorted in the looking-glass. I was sent for, and found the mouth drawn very much to the left side and the muscles on the opposite side flaccid and powerless; the tongue on being protruded was turned to the right side; she had some headach, and was very much frightened; pulse 98 and small; leeches were applied behind the right ear, and after free purgation she was quickly brought under the action of mercury. Leeches were several times applied in the same situation, followed by blisters, under which treatment the paralysis of the side of the face gradually diminished, and in less than three weeks it had quite disappeared. She was confined on the 21st of the month. Her labour was easy, of eight hours' duration, and the child, a girl, was born alive and healthy. The urine was not examined. This lady has had two children since, and suffered no deviation from the healthy state in either pregnancy.

CASE XXI.—*Hemiplegia*.—August 8, 1844. Mrs. —, first pregnancy. This lady, very young, very small, of a lively active temperament, was seized with paralysis of the right side

^a Cases in Midwifery, &c., by J. J. Crosse, M.D., F.R.S., p. 163. ^b Ibid. p. 164.

of the body, in the middle of the night, just three weeks before her confinement at the full period. The attack commenced with a fit resembling epilepsy or convulsive hysteria. When I saw her the next day she could not speak intelligibly, and had very little power over the right leg and arm. Her face was flushed. She complained much of headach, and was very irritable. Her pulse was 98, small and weak. Leeches were immediately applied to the temples, and the bowels were well freed as soon as possible, a large quantity of dark fæces being expelled. When this was effected, she was rubbed with mercurial ointment, and leeches were again applied to the temples and behind the ears, followed by blisters to the nape of the neck and behind the ears. She was quickly brought under the influence of mercury, and severe salivation ensued. The power over the limbs soon began to return, and were it not for the severity of the action of the mercury on the mouth, she could have spoken. She recovered perfectly in a fortnight, and was able to walk about and use her legs and arms. Her mouth continued very sore until her delivery took place. Her labour was an easy one, lasting only six hours, when a fine healthy girl was born. This lady had no swelling of the limbs nor any other premonitory symptoms of a convulsive attack. The urine was not examined. She has borne three other children since without any unnatural occurrence.

CASE XXII.—*Partial Paralysis of Right Side.*—My friend, Dr. McClinton, has favoured me with the following case:—

“A lady, of healthy constitution, but nervous temperament, was suddenly seized with numbness, coldness, and partial loss of power of the right leg and arm, when in the last month of her fourth pregnancy. She was immediately put to bed, and the limbs well chafed with spirits. I saw her very soon after the attack, and found her in a state of great alarm and nervousness. Neither pain nor vascular fulness of the head was present; nor had she suffered from any symptom usually regarded as indicative of cerebral congestion. In the course of an hour she lost the unpleasant sensations in the limbs, and completely regained the power of them. The following day her only source of complaint was an unpleasant tingling in the ring and little finger of the right hand, and at times also in the right side of her tongue and lips. In the course of the next three weeks she had occasional returns of these anomalous sensations in the leg, arm, and tongue, but less often in the leg than in the other parts. Once or twice she complained of her forehead and the roof of her mouth being similarly affected. On the 11th of August (1849), she was confined, and had a

short and easy labour, and a most favourable convalescence. On the fourth day there was a return of the numbness and coldness, &c., as before, in the right leg, side, and arm. Between this date and the 1st of October, when her child, which she had been nursing, died, seldom more than three or four days passed without a visit from her troublesome complaint. It lasted about an hour, and was generally accompanied by a considerable flow of limpid urine. On more than one occasion I observed that the temperature of the affected limb was lower than that of the other; once, but only once, both legs were affected. Dr. Chas. Johnson saw this lady with me on Sept. 13, and agreed with me in thinking that her symptoms were of a purely nervous kind. Menstruation came on a few weeks after the death of her infant; nevertheless she continued to be tormented with this deranged sensation of the right side of the body. In the middle of December she went by our advice to the country, and returned home again in six weeks, wholly and entirely free from her complaint, after its having persisted for five months under the varying conditions of pregnancy, the puerperal state, lactation, and menstruation.

II.—PARALYSIS DURING AND AFTER DELIVERY.

CASE XXIII.—*Amaurosis*.—Mrs. —, aged 26, was seized in labour of her first child, September 9, 1811, and was soon after attacked with convulsions. The fits were frequent and violent, and continued less frequently after delivery, which was completed by the forceps; she was bled largely; blistered; cold applied to the head, &c.; but she remained insensible forty-eight hours after delivery, after which she gradually recovered. "She was left completely blind for two weeks; she then began to see imperfectly, but was six weeks before she could distinctly discern objects"^a. In another case of convulsions, related by the same author, the sight, especially of one eye, remained for some time imperfect.

CASE XXIV.—*Amaurosis*.—"Mrs. C., first pregnancy; under difficulties and depressed spirits; under thirty years of age; labour began with a convulsion; eyesight and sensibility lost; pupils greatly dilated. I was called in in consultation; the os uteri was much dilated; delivery effected by forceps. Bleeding and blistering being freely practised, the convulsions ceased; eyesight, quite lost for several days, at length returned, and there was perfect recovery"^b.

^a Dewees' Compendious System of Midwifery, p. 505.

^b Crosse's Cases in Midwifery, &c., p. 155.

CASE XXV.—*Hemiplegia*.—I am indebted to Dr. M'Clin-tock for the following:—"This case occurred shortly before my leaving the hospital in the year 1847. E. D., aged 36, was delivered of a healthy boy, her third child, after an easy labour of about four or five hours' duration. Paralysis of the right arm and hand came on in the course of labour, and was wholly unattended by convulsion or any cerebral affection. She recovered the effects of her accouchement most satisfactorily, and could not be prevailed upon to remain in hospital beyond the eighth day (the usual time for the patients to return home) although she had but partially regained the use of the affected limb. During the seven days she remained under observation the treatment employed was at first warm stimulating fomentations to the arm, and afterwards blistering along the course of the brachial nerves, together with active purgatives. A very marked improvement took place under the use of these means, but the ultimate result of the case I do not know."

CASE XXVI.—*Paraplegia*^a.—"In the month of December 1850, M. J., aged 32, an unmarried servant, was admitted into the Wurzburg Lying-in Hospital. She had previously been twice confined after natural labours, the last time in July, 1848. Eight days after this last confinement, she caught cold as she was washing in water up to her knees. Two hours afterwards she was attacked with symptoms of paralysis of the lower part of the left leg, which in the course of some days extended to the left thigh, and after two or three weeks to the right leg below the knee. At this time also the movements of both upper extremities were somewhat difficult, yet, though the patient could neither stand nor walk, she could still employ her hands in knitting, sewing, &c. In the month of May, 1850, conception again took place, accompanied by an increase of the palsy, without any diminution of the sensibility of the affected parts in the course of the disease. On the other hand, the deficiency of nutrition in the muscles of the forearm and legs was remarkable." Labour set in January 28, 1851, and after a considerable time she was delivered of a healthy child. She recovered well, and the paralysis seemed to diminish slightly from the fourth to the tenth day. Local bleeding, blistering, electricity, strychnia, and ergot, were tried without material benefit, and she remained, a year and a half later, much in the same state as when she left the hospital.

CASE XXVII.—*Paralysis of Right Leg*.—The following

^a Scanzoni, *Lehrbuch der Geburtshilfe*, p. 1000.

case has been published by Dr. Beatty^a. “Anne Kiernan, aged twenty-one, delivered of her first child, November 26, 1836, after a labour of seven hours; infant born alive. Nothing remarkable occurred during labour or afterwards, until she complained on the second day that she could not move her right leg, and that it felt benumbed and dead. On examining the limb, no swelling nor pain could be discovered at any part that could indicate the approach of phlegmasia dolens; on the contrary, the sensibility of the limb appeared considerably lessened. Frictions with warm turpentine were ordered to the limb, but without any effect upon the condition of the part. At the end of a fortnight, finding that no improvement had taken place, a course of blisters along the line of the sciatic nerve was commenced, beginning above and going downwards. This plan, together with attention to her general health, had the effect of gradually restoring the power of the limb. In a month she was able to walk across the ward with the assistance of a stick, but even yet the leg was dragged along with difficulty, and when carried forward, the foot hung loose and vacillating, the toes pointing to the ground. In another month, she had regained considerable power over the muscles, her progression was much more firm and steady, and the sensibility of the limb was almost entirely restored. She continued to improve until the month of February, at which time she was walking about nearly well, and preparing to leave the hospital, when puerperal fever made its appearance in our wards.” She was attacked by pericarditis, and died in about a week.

CASE XXVIII.—*Paralysis of the Left Leg*^b.—“In the month of February, of the present year, 1851, a woman, aged 33, applied at the Polyclinique: on the 25th of January she had been delivered of her third child by the forceps, after a heavy labour, which had lasted twelve hours. During parturition she suffered from painful spasms of the left leg, and on the following and subsequent days, after she had left her bed, complained of lassitude, difficulty of walking, and diminished sensibility of the left foot. The examination showed that the sensibility of the left leg and thigh was normal, but that it was deadened on the dorsum and in the sole of the foot, so that the patient could not distinctly feel the hand when passed over it, or the ground when she put down her foot. The diminution of motility was betrayed by a laborious dragging of the leg in walking, and by

^a Second Report of the New Lying-in Hospital: Dublin Journal, First Series, vol. xii. p. 304.

^b Romberg on Diseases of the Nervous System, Sydenham Society's Edition, vol. ii. p. 390.

the difficulty with which she executed all the movements. The veins were varicose, and the uterus had remained prolapsed after delivery. A purgative was ordered, followed by friction with oil of turpentine and the internal exhibition of the spirituous extract of *nux vomica*, commencing with half a grain and increased to one grain three times a day. The result was so completely satisfactory, that the motility and sensibility were entirely restored, and on March 3rd the patient was discharged cured."

CASE XXIX.—*Hemiplegia*.—For this case I am indebted to Surgeon Forrest. Mrs. H., aged 29, was confined of her second child on Friday, June 10, 1853, after a natural labour of about five hours, the second stage being short. Considerable hemorrhage occurred after delivery, producing fainting, &c., when Mr. Forrest was called in consultation. By means of a compress beneath the binder, and the application of cold, the discharge was controlled, and the patient progressed favourably, with abundance of milk, and the lochia natural, until Tuesday, June 14, when the lochia ceased without any apparent cause, and without uneasiness of any kind until Friday, June 17th, the eighth day after her confinement, when she was seized with paralysis of the right leg and arm, without headach or any other premonitory symptom. The muscles of the face were unaffected, the sight and speech were perfect, the pupils natural, and the intellect intact. On the following morning, June 18, she had a severe attack of convulsions, which affected the entire body, and the paralyzed extremities as well as the others, after which Mr. Forrest found her in a state of stupor. She had eight fits on this day. On Sunday she continued in a state of stupor, from which, however, she could be roused, and on this day also the convulsions recurred, though rarely, after which they ceased altogether. The pulse was quick, the intellect before and after the convulsive attack was clear, the speech perfect, the eyes natural, the sensibility of the paralyzed limbs unaltered, but the motor power entirely lost. After the convulsions she complained of headach, but this gradually ceased, and she recovered by degrees the use of her arm and leg in about two months. No external cause could be discovered for the attack; she had neither anxiety nor shock, was in good health previous to labour, and was neither liable to headach nor hysterical attacks. The hemorrhage after labour precluded blood-letting, so that the treatment consisted chiefly of counter-irritation by mustard cataplasms, turpentine and assafoetida enemata, four-grain doses of camphor every second hour, purgatives, &c. On Saturday, June 18, Mr. Forrest had

the benefit of Dr. Montgomery's assistance in consultation. The patient is quite well at the present time.

CASE XXX.—*Hemiplegia*.—The following case, which also occurred after hemorrhage, is related in a letter from Dr. Ley to Sir Charles Bell^a: “Mrs. W. was delivered by a midwife at Kilburn. The labour was easy, but followed by profuse hemorrhage upon the separation of the placenta from the uterus. She revived from the state of exhaustion immediately consequent upon the loss of blood, but at the end of about three or four days became feverish, and complained of severe headach; for a week, however, she had no other assistance than that of the midwife. At the end of that time (about ten days after delivery), the headach continuing, and being now accompanied with some degree of ‘numbness on one side,’ I was requested to see her. I found her labouring under severe headach, not confined to, but infinitely more violent upon one side than the other, and occupying the region of the temporal and occipital bones, above the mastoid process, and attended with considerable pulsation. Upon one side of the body there was such defective sensibility, without, however, corresponding diminution of power in the muscles of volition, that she could hold her child on the arm of that side so long as her attention was directed to it; but if surrounding objects withdrew her notice from the state of her arm, the flexors gradually relaxed, and the child was in hazard of falling. The breast, too, upon that side partook of the insensibility, although the secretion of milk was as copious as in the other. She could see the child sucking and swallowing, but she had no consciousness, from feeling, that the child was so occupied: turgescence of that breast produced no suffering, and she was unconscious of what is termed *the draught* on that side, although that sensation was strongly marked in the other breast. Upon the opposite side of the body there was defective power of motion, without, however, any diminution of sensibility. The arm was incapable of supporting the child; the hand was powerless in its gripe; and the leg was moved with difficulty, and with the ordinary rotatory movement of a paralytic patient; but the power of sensation was so far from being impaired that she constantly complained of an uncomfortable sense of heat, a painful tingling, and more than the usual degree of uneasiness from pressure, or other modes of slight mechanical violence. Medicinal agents, including bloodletting, general and local, blisters, purgatives, &c., directed, first by myself, afterwards by Dr. P. M. Latham, to whose care I di-

^a Bell on the Nerves, Appendix, No. 85.

rected her in the Middlesex Hospital, were of little avail, and she left the hospital scarcely, if at all, benefited. At the end of a few months she again proved pregnant. Her delivery at the full time was easy, and unaccompanied with hemorrhage or other formidable occurrence; but at the expiration of about ten days she complained of numbness on both sides. The articulation was indistinct; she became more and more insensible, and sunk completely comatose. Upon examination of the body, no positive disorganization of the brain could be detected. The ventricles, however, contained more than the usual serum; and there were found, more especially opposite to the original seat of pain, thickening and increased vascularity of the membranes, with moderately firm adhesions in some parts; in others an apparently gelatinous, transparent, and colourless deposit interposed between them. Such is the outline of a case which I have been in the habit of quoting in my lectures as an illustration of one of the pathological conditions which I have repeatedly observed as a consequence of great and sudden loss of blood, and as a proof that it is a state of local congestion, allied if not amounting to actual inflammation."

CASE XXXI.—*Paralysis of Face and Arm*.—Mrs. S., aged 43, was confined of her thirteenth child (all of whom are living) in June, 1844. Her labour was perfectly natural, neither preceded, nor accompanied, nor followed by any unusual symptom, until the seventh or eighth day: in the evening of which day, when quietly talking with her husband, she suddenly commenced exclaiming, "Conveniency, conveniency, conveniency." Upon attention being directed to her condition, the mouth was observed to be quite drawn to one side, and complete paralysis of one arm existing. The leg of the same side was not affected. She was not nursing; the lochia were quite natural, and the bowels free. When Dr. Duke first saw her, the only additional symptoms he noticed were, a very quick pulse, and some difficulty of articulation. Cold to the head, aperients, and slight mercurialization, were the remedies employed, and they were successful, for she recovered the use of the arm and the power of speech in a fortnight. The quick pulse continued for some months, together with a certain amount of indistinctness of vision, for which she came to town, and I saw her, in consultation with Dr. Jacob and Dr. Duke. We advised counter-irritation, and a tonic treatment, under which she recovered perfectly.

CASE XXXII.—*Hemiplegia*.—Mrs. K., aged about 38, was delivered of her fifth child, September 15, after a very easy labour. She was a woman of a very fragile constitution, and can

hardly be said to have ever recovered from the inanition caused by incessant vomiting in a former pregnancy. She had not suffered, however, from headach or giddiness, and her stomach and bowels were in pretty good order during this pregnancy: neither had she any œdema or other local complaint. On September 16 and 17, and up to noon of September 18, she continued quite well. The lochia were natural, and there was a sufficient secretion of milk. At noon, September 18, I was sent for, as they thought that she did not seem well, and I found her hemiplegic on the left side. She was not, and the nurse believed that she had not been insensible, and she could speak pretty well, although her mouth was drawn to one side. The motor power of the arm and leg entirely lost, but the sensibility not impaired; pulse 120. A few leeches were applied to the temples, and the head was shaved and blistered: the bowels were too irritable to bear mercury. By these means, and a repetition of the blister, and afterwards the insertion of a seton in the arm, she seemed much relieved. She remained perfectly intelligent, spoke well, gradually acquired the power of moving the leg, and, in a less degree, the arm; her face had recovered its natural expression, and ceased to be drawn to one side; the appetite was good, and the bowels regular; the only symptom which made me uneasy was the quick pulse, which never fell below 100. October 2nd. She felt quite well this morning; as the bowels had been confined, she took a pill last night, and when it acted, she got up to the night-chair; whilst sitting there she became very faint, and never afterwards rallied. She died at 8 P.M., of the same day, without any increase of paralysis, without coma or stertor; in short, without any new symptom. No post-mortem examination could be obtained.

CASE XXXIII.—*Facial Paralysis*.—Dr. Ireland has furnished me with the following record of three cases in one family. Mrs. O., aged 34, was confined of her fifth child, November 28, 1853, and at the end of December was attacked by paralysis of the right side of the face, indistinct vision, ptosis of the right eyelid, &c. Under the influence of leeching, blistering, and mercury, she recovered. Her mother had a similar attack after her confinement, which proved fatal; and her sister had suffered from paraplegia for years, which always increased after her confinements until her death.

CASE XXXIV.—*Hemiplegia*.—Mrs. A., aged 26, was confined for the fourth time on Saturday, November 12, 1853, after a labour of two or three hours, the second stage being under one hour. She had enjoyed excellent health during pregnancy; had no headach or derangement of the stomach or bowels, no œdema; nor was she subject to nervous or hysterical attacks.

She was neither plethoric nor anemic. After her confinement she recovered, without a single drawback up to the seventh day, November 18th, on which day, at 9 A. M., after speaking to the nurse quite composedly, but without making any complaint, she became insensible, with some twitchings of the face, but without any other convulsive movements. The insensibility lasted but a few minutes, but when she recovered she was found hemiplegic on the right side, with some difficulty of speaking. These symptoms gradually diminished, however, and at 3 P. M., when Dr. Duke requested me to see her, she could move both leg and arm, and grasp my hand firmly, and speak quite intelligibly. She was quite intelligent, but there were some words which she either could not pronounce or could not remember, although she recognised them when mentioned, and assented, nor could she put out her tongue freely. She said that she had no pain in the head, or anywhere else; the eyes were clear and bright, the pupils well dilated, and amenable to light, which, however, caused her no annoyance. The pulse was 140, small, thready, and fluttering. There was a slight degree of tenderness in the right iliac region, which disappeared soon after; the lochia were abundant, natural in appearance, and free from unusual odour; and she had plenty of milk. After very minute inquiry, neither Dr. Duke nor I could detect any cause for the attack. Dr. Duke had applied six leeches to the forehead; had given moderate doses of blue pill and opium, which were continued; and had applied sinapisms to the legs, and a blister to the nape of the neck. She continued pretty much in the same state during the day, but in the evening she had another attack of paralysis, accompanied by very slight twitchings of the arm, after which the loss of power was much more complete, although she retained perfect sensibility throughout.

November 19, 10 A. M. She slept at intervals during the night, pulse 140; small and weak. She can still move the leg a little, but the arm scarcely at all; her speech is thicker, and the difficulty of getting out certain words increased; but she shows that she understands everything that is said. The bowels have been moved, and the bladder emptied; but, from the difficulty of moving, she passes all under her, though not unconsciously. The same remedies were continued, the head shaved and blistered, and chicken broth allowed.

November 20, 10 A. M. In much the same state as yesterday, except that her pulse has improved in strength and volume, and is only 120. She has no pain at all, is quite intelligent; the expression of her face calm and easy; she cannot move the arm, but it is quite sensitive; the leg she moves a little. The bowels were moved, and the urine passed. We had this day

the advantage of Dr. Stokes' assistance, and as he concurred in our plan of treatment, the pills of mercury and opium were continued, another blister applied, and a mixture of ammonia, in infusion of orange peel, ordered.

November 21, 10 A.M. Dr. Montgomery visited her with us this day; we found the paralytic affection in the same state as yesterday, but she seemed not quite so well, in consequence of having passed a sleepless night, and from the bowels having been acted on too freely by the mercury. Pulse 120, weak, but fuller and more steady than they were two days ago. Neither Dr. Stokes nor Dr. Montgomery was more successful than we had been in detecting the exciting or the pathological cause of the attack. The pills were ordered to be omitted, and a chalk mixture, with a few drops of laudanum, substituted. Another blister was applied to the head.

November 22, 10 A.M. Our patient seemed better this morning, more lively and intelligent; she can move the leg more, but the arm and hand are quite powerless; the bowels are more quiet, and she takes a little food well. Partly from her inability to use the bed-pan, and partly from her passing both urine and fæces together when she did use it, we had no opportunity of examining the former until to-day. The nurse had always told us that it appeared natural, but this day we procured a quantity, which I brought away for analysis. Unfortunately, the cork came out of the bottle, and all was spilled except about half an ounce. This, though insufficient for an accurate quantitative analysis, was enough to show the presence of a large proportion of albumen, with epithelial scales, pus corpuscles, and the urates of ammonia and soda.

Further visits on my part were unnecessary, but Dr. Duke was kind enough to furnish me with specimens of the urine passed in the nights of November 23rd, 25th, 26th; and my intelligent young friend, Mr. Charles Leet, has given me the following careful analysis of each.

No. 1.—Nov. 23. Urine, pale-yellow in colour, faint, peculiar odour, feebly acid reaction. Specific gravity, 1028·500.

Water,	934·850
Solid constituents,	65·150
Urea,	14·591
Uric acid,	1·250
Fixed salts,	11·166
Albumen,	19·225
Ammonia, salts, and extractive matter,	18·918

Amount in 1000 parts of urine, 65·150

No. 2.—Nov. 24. Physical characters the same as the last, but with a much smaller sediment. Specific gravity, 1024·250.

Water, ...	943·087
Solid constituents, ...	56·913
Urea, ...	18·340
Uric acid, ...	1·200
Fixed salts, ...	9·245
Albumen, ...	10·928
Ammonia, salts, and extractive matter, ...	17·200

Amount in 1000 parts of urine, 56·913

No. 3.—Nov. 26. This specimen was of a deeper yellow colour, and had a stronger reaction. Specific gravity, 1014·500.

Water, ...	969·658
Solid constituents, ...	30·342
Urea, ...	9·250
Uric acid, ...	1·909
Fixed salts, ...	6·100
Albumen, ...	3·833
Ammonia, salts, and extractive matter, ...	9·250

Amount in 1000 parts of urine, 30·342

The following Table will afford a comparative view of each specimen with the others and with the average standard in health. As the quantity passed in twenty-four hours could not be ascertained, the normal quantity, 30 oz., has been assumed:—

	Normal Average.	Specimen No. 1.	Specimen No. 2.	Specimen No. 3.
Amount of urine in twenty-four hours, ...	30 oz.	30 oz.	30 oz.	30 oz.
Specific gravity, ...	1019	1028	1024	1014
Solids, ...	570	840	720	420
Urea, ...	218	188	228	121
Albumen, ...	...	155	134	39

Thus we see that the solid matter in No. 1 and No. 2 is far above the average of health; that the quantity of urea is nearly as much below it, except in No. 2, where it is in excess; and that there is a large proportion of albumen, although diminishing with each specimen. It may fairly be presumed, I think, that the

disproportion of these constituents was even more remarkable at an earlier period of the disease, and for this reason I cannot avoid expressing my regret that I did not bestow more care upon it.

I have said that I did not see the patient after November 22nd, but Dr. Duke informs me that she continued to improve slowly up to November 25th, after which, for a few days, she seemed not so well; her intelligence was less, and she seldom spoke, but answered by a nod or a shake of the head; she retained the power of moving the leg, but not the arm. Nov. 26th. There was barely a trace of albumen in the urine. Nov. 30th. Dr. Duke informed me that our patient is again improving slowly. Dec. 12th. Up to this day the improvement had continued, slowly indeed, but quite marked. Her intelligence was restored, her bodily strength increased, her appetite better; in everything, save the impotence of the arm and leg, she was going on most favourably. During the morning she seemed very comfortable, and was talking cheerfully with her sister. At one o'clock she raised herself to a sitting posture in bed, and took some gruel, feeding herself with her left hand. As she finished, some remark of her sister's excited a fit of hearty laughter, after which she suddenly exclaimed, "Oh dear! Oh dear!" fell back insensible, and expired almost immediately.

Post-mortem Examination, Dec. 14, 2 P.M., forty-eight hours after death, by Dr. Duke and myself.—There were the usual marks of the gravitation of the blood, but no sign whatever of any putrefactive change; the body was in good condition, and a layer of fat, an inch thick, was found on cutting through the abdominal integuments. The head was first examined: there was no turgescence of the scalp, nor, when the skull was removed, was there anything abnormal detected about the dura mater. On removing this covering we found the superficial vessels moderately congested, except at one part of the anterior lobe of the right hemisphere, which was quite pale and bloodless, with a slight effusion of serum beneath the arachnoid. But the most memorable fact noticed at this stage of our examination was, that every blood-vessel contained bubbles of air, alternating with globules of blood, giving to each vessel a beaded appearance, and this extended to very minute vessels, and to those in the division between the hemispheres. We traced the blood-vessels as minutely as we could with the naked eye, but could discover neither obstruction nor obliteration. The brain was then carefully removed; the upper portion of the spinal marrow and the nerves appeared quite healthy; there was no morbid appearance about the base of the brain; the pons varolii

and the parts adjacent exhibited neither congestion externally, nor bloody points when cut into, nor any change in the firmness or appearance of their structure; the right hemisphere was healthy throughout, of its usual firmness and appearance, and, when divided, free from vascular points. In the anterior lobe of the left hemisphere, just about the anterior termination of the ventricle, we found the white cerebral substance, and, to a limited extent, the gray matter in the neighbourhood, reduced to a pulpy condition, about the density of gruel; the texture was completely destroyed for about an inch and a half in length by half an inch in breadth; the colour was very little changed, was certainly not redder than usual; posterior to the diseased part the tissue seemed quite natural; there was no hardness nor vascularity,—nothing, in short, to mark the transition from diseased to healthy structure. Again, in the posterior lobe, there was a similar, but smaller, spot of softening, without surrounding vascularity or hardness. We remarked, indeed, that the bloody points generally seen upon cutting through the substance of the brain were less numerous than usual. Dr. Lyons examined a portion of the softened part, and he found nothing but exudation corpuscles, with the debris of cerebral fibres; neither purulent nor serous infiltration; no other morbid appearance was discovered in the brain or cerebellum, and there was not above an ounce of serum escaped.

The lungs were free from adhesions, and perfectly healthy.

The heart was of the usual size, its walls of the ordinary thickness, and its cavities normal and empty; the auriculo-ventricular and aortic valves were complete, perfect, free from vegetations, and of the usual thinness.

On opening the abdomen we found no trace of peritonitis; the stomach, the greater portion of the small, and all the large, intestines, were perfectly healthy; in one part of the small intestines we found the coats stained of a reddish-brown colour, and the mucous membrane quite softened and pulpy.

The same reddish-brown colour extended to the contents of the pelvis; we found the uterus nearly reduced to its natural size (five weeks after delivery); its walls were of their natural thickness, and apparently healthy; the cervix was dark-coloured, and had still a bruised appearance; the cavity contained a thick, gelatinous, reddish-brown fluid, of which some had escaped through the vagina on to the bed; it had no putrid odour, but resembled not quite healthy menstrual fluid; the ovaries were small and healthy, but the broad ligaments and Fallopian tubes retained an unusually vascular appearance, and in the folds of the ligament was a cyst as large as a grape.

The kidneys were dense, and one much larger than the other; when cut into they exhibited* great congestion, and from the divided tubes purulent matter escaped.

The other viscera were perfectly healthy.

1. I shall now give a summary of the foregoing cases, and make a few remarks upon the more important points connected with them. With regard to the numerical value of the cases, I should wish it to be understood that I consider the cases far too few to enable us to draw any very decided conclusions, although, as far as they go, the results are worth stating.

Of the 34 cases, in 22 the attack occurred during pregnancy; in 12, either during or after labour.

In 23 cases where it is mentioned, I find that with 10 it was their first child; with 1, the second; with 4, the third; with 2, the fourth; with 3, the fifth; with 1, the sixth; with 1, the thirteenth; and 1 had several children, but the number is not specified.

Of the 34 cases, there were 17 of complete hemiplegia, and 1 partial; 4 of paraplegia, in 2 of which only one leg was affected; 6 of facial paralysis; 5 of amaurosis; and 3 of deafness; but in some of these latter local palsies were combined with the cases of hemiplegia. Of 14 cases of hemiplegia, in which the side affected is mentioned, I find that 11 were of the right, and 3 of the left side.

Of the 34 cases, 4 died.

It may be well, however, to consider these cases somewhat more closely, and for that purpose they may be divided into two classes, those which occurred during pregnancy, and those which were attacked during or after labour.

2. Of the 22 cases in which paralysis occurred during pregnancy, 12 were examples of hemiplegia; 1 of paraplegia, which had occurred previously; 4 of facial paralysis; 2 of amaurosis; and 3 of deafness. There is no regularity as to the period of gestation at which the seizure took place, for of 13 cases in which this is mentioned, in 1 it occurred in the second month; in 1 in the third or fourth; in 1 in the fifth; in 1 in the sixth or seventh; in 3 in the seventh; in 2 in the eighth; and in 4 in the ninth month; from which it would seem, upon the whole, that it is in the latter months that pregnant women are most liable to the attack.

Of 19 cases, 11 appear to have been cured before or by delivery, and in 8 the disease continued for a longer or shorter time afterwards.

Of the 20 cases, only 1 died, and in this case it is evident that death was rather owing to disease of the brain, of longer

standing than the pregnancy, than to the paralysis which increased during that process; so that I do not think we can reckon it as impairing the comparatively innocuous character of these attacks during gestation.

In 3 cases only was the paralysis preceded by convulsions. In most of the cases it does not appear that there were any premonitory symptoms, little or no headach, or any other circumstance calculated to excite apprehension until the paralysis supervened. The characteristics of the palsy resembled very closely those of similar attacks unconnected with pregnancy: the motor power was enfeebled or altogether lost; in some the sensibility was increased, diminished, or modified; but in others, I infer from the silence of the reporter, that it was little, if at all, changed from its natural condition. The intellect seems to have preserved its integrity in all the cases. A peculiarity of great interest in many of these cases, and to which I shall revert by and by, is the presence of albumen in the urine, whenever that secretion was carefully examined.

3. The second class, consisting of 12 cases, is characterized by the attack occurring during or after labour. It is remarkable that in 3 cases only (Cases *xxiii.*, *xxiv.*, *xxv.*) did the paralysis take place during labour, and of these, 2 were cases of convulsions; in all the others it not merely succeeded labour, but in most cases after an interval sometimes considerable: for example, in Case *xxiii.* it took place on the first day after delivery; in Case *xxvii.*, two days afterwards; in Case *xxxii.*, three days; in Case *xxxiv.*, seven days; in Cases *xxvi.*, *xxix.*, *xxxi.*, eight days; in Case *xxx.*, ten days; and in Case *xxxiii.*, a month afterwards.

Of these 12 cases, 5 were cases of complete hemiplegia; in 1 only the arm was affected; 1 was a case of complete paraplegia; in 1 the right, and in 1 the left leg only was paralyzed; 2 were examples of amaurosis; 1 of facial paralysis; and in 3 only of the cases of hemiplegia the face participated in the attack. In Dr. Ley's very remarkable case, the paralysis of the motor power of one side was accompanied by loss of sensibility on the other. In some of the cases the sensibility was diminished, in others unaltered, but in none increased. The phenomena of the disease were not peculiar: in the majority of the cases the attack occurred generally without warning, and without any obvious cause. In 2 cases convulsions terminated in amaurosis, but in Mr. Forrest's case the paralysis preceded the convulsions, and during the latter, the paralyzed limbs shared in the convulsive movements.

The duration of the disease varied a good deal, the para-

lysis gradually subsiding in most cases: in Case xxii., after several days; in Case xxiii., in six weeks; Case xxxi., recovered the use of the arm in a fortnight, but vision remained imperfect for some months; in Case xxxiv., in a month; in Cases xxviii., xxix., in two months; Case xxii., recovered the power of walking in two months, but was then attacked by another disease which proved fatal; Case xxvi. left the hospital without improvement.

In 3 cases death occurred: in Case xxxii., on the fourteenth day, and in Case xxxiv., on the twenty-fourth day after the paralytic seizure. Dr. Ley does not mention on what day his patient died.

4. I have already alluded to the fact that in most of the cases the attack occurred without warning, and without apparent *cause*. Some cause there must be, of course, but it is much easier, in most cases, to say what it is not than what it is. For example, in none of these examples except one, did it appear to depend upon any external influence,—upon cold, exposure, violence, &c.,—or upon mental distress; in few, if any, was there evidence of previous cerebral congestion, or disease of any other organ.

It has been suggested that the palsy may be merely the termination of convulsions, and certainly some of these cases would seem to support this view; but if this were generally true, we should find convulsions more frequently preceding the paralysis, and, also, we should meet with more cases of convulsions terminating in paralysis. Now, in all the cases I have quoted, a large majority exhibited no convulsive movements at all, and, on the other hand, of all the cases of convulsions related by Drs. Collins, and M'Clintock and Hardy, there is not a single instance of such a termination; we must therefore refer both convulsions and paralysis to some common or different cause.

I have no doubt, as Dr. Romberg has observed, that in a number of cases, especially those which occur during gestation, the palsy is due to a reflex action from some organ or structure in a morbid condition, and in which the nervous system seems to be merely the channel of transmission, offering no central disorganization. In such cases the exciting cause may possibly be some injury or morbid condition of the generative organs, or perhaps merely a transient excitement, such as that of pregnancy. It is possible, also, that some of the instances occurring during gestation ought rather to be classed under the head of hysterical paralysis, as described by Drs. Laycock and Romberg, but it is not always easy to make the distinction.

Obstruction of the arteries has been recently shown by Professor Simpson^a, to be an occasional occurrence in child-bed, either from arteritis, a coagulum, or a detached vegetation: and a degree of paralysis, may be the result; but inasmuch as the death of the limb, and ultimately of the patient, is the direct consequence of such an occurrence, the history of the cases I have quoted removes from them the suspicion of being thus caused.

It might naturally be supposed that the stress and exertions during labour which give rise to such great congestion of the face and head, by also occasioning congestion of the brain, might be considered one of the principal causes, but such a supposition is not borne out by facts, for, excluding the cases of convulsions, in only one case did the paralysis occur at the time of labour; in all the others it either supervened before labour, or subsequently, at a time when all such direct action must have ceased, and in some, after such an interval that we cannot suppose it even a remote effect of the parturient agony. On the other hand, when we remember the number of severe labours in which no such attack occurs, or compare its frequency with that of convulsions during labour, we can scarcely attribute much influence to this cause.

Again, as we have seen, paraplegia has been attributed to severe and prolonged labour, and to the consequent mechanical pressure upon the nerves and muscles of the pelvis, and at first sight this seems an adequate and feasible explanation, and of which no one could deny the possibility; yet so far as our cases are concerned it can hardly have been so, for in all but one the labour was natural, easy, and not prolonged: in the exceptional case the patient had been delivered by the forceps; moreover, the period at which it occurred was too distant to justify our attributing it to this cause in the other cases. On the other hand, if we recollect the number of severe, prolonged, and instrumental deliveries which take place, without any such result, no example being recorded by Drs. Collins, M'Clintock and Hardy, or, with the exceptions I have quoted, in any of the reports of the British and foreign hospitals, so far as I am acquainted with them, I think we must also reject this peculiarity of labour as a necessary or frequent cause.

In two cases the attacks seem to have been connected with an anemic condition, consequent upon hemorrhage, either from the direct effect of a deficiency of the circulating fluid, or indirectly from the increased susceptibility of the nervous system, under these circumstances, to ordinary exciting causes.

^a Edinburgh Monthly Journal, February, 1854.

In another case paraplegia appeared to result from cold; but, in the majority of cases, as I have already observed, there was neither plethora nor anemia; neither exposure, want, injury, advanced age, mental distress, nor sudden shock; in short, there was no apparent cause.

5. Unfortunately for the cause of science, there are very few post-mortem examinations on record, from which we might decide with some degree of certainty upon the nature of the affection. In all the slighter and more partial cases, life is preserved, and when death occurs in the more severe instances, permission to examine the body cannot always be obtained. Of the four fatal cases I have here detailed, two only were examined: in these, and I doubt not, in the other two also, disease of the brain or its membranes existed. In Dr. Ley's case, he states that, "no positive disorganization of the brain could be detected. The ventricles, however, contained more than the usual serum; and there was found, more especially opposite to the original seat of pain, thickening and increased vascularity of the membranes, with moderately firm adhesions in some parts; in others an apparently gelatinous, transparent, and colourless deposit, interposed between them." In short, there appears to have been an attack of partial meningitis, and the contrast between the peculiar train of symptoms to which it gave rise, and the absence of all symptoms except the palsy in Dr. Duke's case, is very interesting, when we remember the remarkable disorganization we discovered in the latter case.

Now in these cases we may fairly assume that the palsy and death itself were the result of the disease of the brain and its membranes, but to what are we to attribute the slighter and more numerous cases? Do they not appear to belong to the class described by Dr. Abercrombie, as "depending upon a cause which is of a temporary nature, and capable of being speedily and entirely removed."

6. What is this temporary cause, producing so serious a disturbance, and yet scarcely, if at all, endangering life? May it be the one to which Dr. Latham refers, as observed "in those convulsions and apoplexies which appear and disappear, the chief circumstance which attracts our attention being albuminous urine"? At any rate it deserves our careful attention. Of the fact of the concurrence of albuminuria with certain affections of the nervous system during pregnancy and childbed, there can be no doubt whatever. Both Drs. Lever and Simpson have detected it in cases of convulsions during pregnancy and labour: the former observes:—"I have carefully examined the urine in every case of puerperal convulsions that has since

come under my notice, both in the Lying-in Charity of Guy's Hospital, and in private practice, and in every case but one, the urine has been found to be albuminous at the time of the convulsions." "I have further investigated the condition of the urine in upwards of fifty women, from whom the secretion has been drawn during labour by the catheter, care being taken that none of the vaginal discharges were mixed with this fluid; and the result has been that in no cases have I detected albumen, except in those in which there have been convulsions, or in which symptoms have presented themselves, which are readily recognised as precursors of puerperal fits." Dr. Simpson's observations about the same time, and those of more recent observers, Sabatier, Legroux, Richelot, and others, have confirmed the conclusions of Dr. Lever as to the presence of albumen in the urine in cases of puerperal convulsions, so that no doubt now exists as to the fact, although we occasionally meet cases of convulsions without albuminous urine, and of albuminuria without convulsions^a.

Now, as paralysis in some cases occurs in connexion with convulsions, if not as a consequence of them, we might, not unnaturally, expect albumen in the urine of such patients, and accordingly, in a patient of Dr. Lever's, and in others, we find that it has been detected.

But we may go a step further, and state that in cases where no convulsions have preceded the paralysis, albuminuria has been equally observed. Dr. Lever says of his cases, that in none in which he examined the urine did he ever fail to find albumen, and the great experience of Professor Simpson is in close accordance with this, as may be seen by the quotations I have given, and by the cases with which he has favoured me. This was observed also in Dr. Duke's case, where the paralysis succeeded the delivery; and in which I think there is ground for believing that the albumen had diminished at the time the urine was first examined. In all probability it would

^a It may be of interest to append Dr. Seyfert's conclusions on this subject:—

"1. Albuminuria is not an essential accompaniment of normal, healthy pregnancy. 2. The theory, ascribing albuminuria to the pressure of the enlarged uterus on the renal vessels, is inadmissible. 3. When anasarca, from Bright's disease, occurs during pregnancy, the patients are seldom attacked by eclampsia. 4. The albuminuria, in cases of eclampsia, is occasioned by the interruption of the functions of the respiration and circulation by the attack. 5. In such cases the albuminuria terminates with the attack. 6. Albuminuria is not present in all cases of eclampsia. 7. Albumen is found in large quantities in the urine of epileptics, *immediately after an attack*; but not invariably after every seizure, or in every case of the disease. 8. Provided there be no Bright's disease, this albuminuria among epileptics ceases soon after the convulsions, and only returns after the next attack."—*Edinburgh Monthly Journal*, Feb., 1854, p. 168.

have been detected in many others, had an investigation been made.

Thus we find that albuminuria may be a marked symptom in puerperal convulsions, whether terminating in paralysis or not; and in the palsy of pregnant and puerperal women, whether partial or complete, whether local or general: and if the observations are yet too few to draw any very positive conclusions, it is, I believe, because our attention has not been drawn to the subject. And when, in addition, we find, as Dr. Lever states, that as the albumen diminishes, the paralysis subsides, we can hardly doubt that there is some important connexion between them.

What then is the precise pathological significance of albuminuria? We may assume as established, that although it occurs in Bright's disease, *it alone is no proof of the presence of that disease*; but in the present state of our knowledge it is very difficult, perhaps impossible, to come to any very decided conclusion upon the matter. It is conceivable that an unusual, morbid, or noxious ingredient in the urine may be produced in either of three ways:—1. By simple elimination from the blood, in which it was present; 2. As the result of diseased action of the kidneys, excited either by some noxious principle in the blood, or by a morbid condition of these organs; or 3. As a new compound, the result of chemico-pathological action, which we may or may not be able to explain.

Now, albumen in the urine cannot be placed under the latter category, as it is not a new principle, but one already existing in the blood. Nor does it come under the first, for although it is possible that it might be eliminated from the blood in which it is present, it cannot be as a noxious element, nor would this simple elimination account for the condition of the kidneys or for the concomitant symptoms. So that it would appear this secretion of albumen must be owing to some disordered action of the kidneys, excited by some morbid element, in kind or degree, which they are endeavouring to separate from the blood. This seems at least to be the opinion of a high authority, Dr. George Johnson, of London, who, in describing acute desquamative nephritis, in which albumen is so largely secreted, observes, "that all the changes of structure commence in the secreting cells of the gland, and are the result of an effort made by the cells to eliminate from the blood some abnormal products, some materials which do not naturally enter into the composition of the renal secretion"^a. This view is further con-

^a Diseases of the Kidney, p. 105.

firmed by a post-mortem examination into the state of the kidneys themselves in albuminuria. Dr. Handfield Jones, in a recent paper, has described three varieties: "The first is the condition of engorgement, such as is seen in those who die in the early stages of acute anasarca, or in that of dropsy succeeding scarlatina. The organ is enlarged, dripping with blood in every part; its tissue not destroyed, but many of the tubes are seen, under the microscope, to contain coagula of exuded fibrine, entangling blood globules, and more or less of epithelium." "The second form of diseased renal structure is that of the large, heavy, often mottled and pale kidney. In this there is no hyperemia, but rather the reverse state usually exists. The cut surface has not the appearance of healthy structure, and gives one the idea of some matter having been implanted among the natural constituents, so as to obscure them and to produce a confused aspect. The tubes are found impacted with epithelial matter, but not by any means constantly obstructed or blocked up, although they may be irregularly dilated, &c." "The third variety of morbid change is that so familiar to observation as the dwindled, granular kidney"^a.

When we consider the temporary nature of the albuminuria in many of the cases of paralysis, we need have little doubt that the condition of the kidneys answers to the first variety here described, or that of extreme congestion, and this opinion is confirmed by the examination of Case xxxiv., in which we found a high degree of congestion, which had indeed passed into a more advanced stage. I think, therefore, that we may fairly assume the albuminuria is due to a congested state of the kidneys, and I confess I cannot but think that the explanation given by Dr. G. Johnson and others, that this congestion is excited by the effort to eliminate some noxious element from the blood, is more in accordance with our present knowledge than any other, yet I must not omit to mention that by some this congestion has been attributed to pressure of the gravid uterus upon the renal vessels. Dr. Seyfert, as we have seen, rejects this mechanical explanation, and seems to attribute the albuminuria to the eclampsia, in consequence of the interruption of the functions of respiration and circulation.

But, if the former theory be true, what is this morbid element, morbid in kind or degree? It is very difficult to answer this question. Dr. Simpson suggests that it may be an excess of urea or some morbid quantity or quality of caseine in the blood. Dr. George Johnson's observations seem to prove that

^a Medical Times and Gazette.

in these cases, in addition to a change in the proportion of the normal constituents of the blood, of which the diminution of its albumen is one, there is always an excess of urea.

Then it may be asked, "to what is the effect upon the nervous system owing?" One can conceive that it may result either—1. From the continued presence of the noxious principle in the blood; or, 2. From the balance of the constituents of the blood having been destroyed; or, 3. From the diseased condition of the kidney,—though to which of these we ought to attribute it, would be difficult to decide.

But at whatever conclusion we arrive with respect to these interesting points, I am sure all will agree with me, that, taking the circumstances into consideration, it is probable the kidneys play a more important part in these paralytic affections than has been suspected, and that the subject deserves more attention than it has received. For, we find that in cases of convulsions terminating in paralysis, we may have albuminuria; in paralysis before delivery, without convulsions, we may have albuminuria; in paralysis occurring after delivery, we may have albuminuria; and further, that in the slighter cases, both the convulsions and paralysis diminish with the decrease of the albuminous secretion. Whether therefore the paralysis be caused by the state of the kidneys; or the renal congestion and paralysis be both the result of some morbid matter in the blood circulating through the system; it is clear that a new element may be added to those which have usually been considered as giving rise to paralysis.

7. Nor is this barren theory only, but, if it be true, it has a direct bearing upon practice, inasmuch as our attention ought not to be confined to the secondary affection of the nervous system in such cases, but must be directed to the relief of the renal malady, and to the restoration of the kidneys to such a state of efficiency as may enable them to remove the morbid constituents of the blood; and for our encouragement, we have seen that a diminution of albumen in the urine is followed by mitigation and cure of the paralysis. For the latter affection, blood-letting, general when the system will bear it, or local by means of leeches or cupping; blisters, purgatives, and mercury, are the remedies usually employed; these must be modified according to the condition of the patient, the circumstances of the attack, and the duration of the disease. When much blood has been lost during labour, bloodletting must be omitted, and we must confine ourselves to counter-irritation; perhaps a series of small blisters to the neck, down the spine, or along the limb, will be the best mode of proceeding. The

patient's strength must be supported judiciously by good diet, and it is quite possible that some stimulant, such as ammonia or camphor, may be necessary. When the paralysis has become chronic, strychnia or galvanism may be found useful: and I believe Dr. Stokes has found galvanic acupuncture very beneficial in facial paralysis.

The renal disorder should never be treated by diuretics, but by external irritants, such as mustard poultices, or rubefacient liniments to the loins, and internally by diaphoretics, as suggested by Dr. Osborne^a, of this city, and when more chronic, by gallic acid, iron, &c.

Before concluding, I will shortly direct attention to some peculiarities in the case which was the immediate cause of my bringing the subject forward. The points of peculiar interest are, the state of the brain itself, the air in its vessels, the condition of the kidneys, and the immediate cause of death.

1. *The Softening of the Brain.*—Of the various premonitory symptoms mentioned by Abercrombie, Lallemand, Rostan, Durand-Fardel, Rowland, &c., such as headach, diminished or modified sensation, loss or weakening of intelligence, &c., our patient exhibited none: up to the occurrence of the paralysis she had been entirely free from either local or general distress; she was also exempt from most of the symptoms which usually attend or follow the attack itself: she had no headach; no twitching of the muscles, except for a few seconds at the first seizure; no rigidity of the flexors; no disturbance of any special sense, or of sensation generally. The face had its natural placid expression; the eyes were intelligent, and sensitive (but not too much so) to light, and the ears to sound; her respiration was easy, and not hurried; her pulse was very quick; and either her memory of some words, or her power of pronouncing them, was imperfect; but these, with the loss of power in the arm and leg, were the only symptoms when I saw her, and even these diminished after a time. Again, of all the exciting causes usually enumerated,—such as hereditary predisposition, temperament, age, occupation, shocks, mental distress, abuse of spirituous liquors, cold, disease of the heart, lungs, digestive system, or kidneys,—there is but one (disease of the kidneys) of which we had the slightest ground for believing that it could have had any influence whatever in the production of the disease. After the most searching investigation during life, it was impossible to discover any apparent

^a On the Nature and Treatment of Dropsies, &c. 1837.

cause. So far, then, the case is certainly peculiar: with none of the usual exciting causes; without premonitory symptoms; and the actual symptoms limited to the loss of power, a rapid pulse, and a slight difficulty of utterance; it seems to answer better to Dr. Rowland's description of "latent softening" than any other^a. Again, if we take the appearances on dissection, they differ a good deal from those in the ordinary forms of acute softening. The colour of the white cerebral substance was but little changed, and certainly it was not reddened; the neighbouring parts were not more vascular than usual; if there was any difference, I should say that there were fewer red points than ordinary when cut across; there was not the slightest induration of the tissue surrounding the softened portion, nor had the latter any very defined limits, but seemed to pass gradually into the healthy tissue; there was neither infiltration of serum nor purulent matter, but merely exudation corpuscles and the debris of cerebral fibres. These appearances resemble pretty closely those of "white softening," as laid down by Dr. Rowland, or those of "chronic softening," by Dr. Gross^b.

Lastly, the duration of the disease was somewhat prolonged for a case of acute softening. From the observations of Lallemand it appears that nearly one-half die within the first seven days from the attack; two-fifths at the end of the second week; and the remainder at intervals of one, two, or three months. Of 59 cases of acute softening, examined by Durand-Fardel, 11 terminated fatally within the first forty-eight hours, 15 before the fifth day, 17 before the ninth day, 7 from the ninth to the twentieth day, and 9 from the twentieth to the thirtieth day. As to the duration of chronic softening, Andral states that out of 105 persons, upwards of 16 survived the first month, more than 10 the second month, 7 reached the end of the third month, and 2 lived for three years.

What, then, was the nature of the softening in this case: and first, was it inflammatory or non-inflammatory? The history of the case, the absence of the usual symptoms, and of the ordinary post-mortem appearances in the brain, would rather lead to the conclusion that it was not the result of inflammation; and, if not, what state of the brain or its vessels gave rise to it? There was no evidence of fatty degeneration revealed by the microscope, nor could we detect obstruction of any vessel by the naked eye; although the state of the brain, I think, answers better to those cases where softening has fol-

^a Pathological Anatomy, p. 355.

^b Ibid.

lowed obstruction and atrophy, than to those in which it resulted from inflammation or fatty degeneration^a.

A second question may be raised, which it is not easy to answer, viz.:—Was the softening acute or chronic? did it occur before the paralysis; or was the latter due to some other cause, which, continuing to operate, produced also, after a time, the *ramollissement*?

2. Now let me draw attention to—the *State of the Blood-vessels on the surface of the Brain*.—With the exception of the anterior lobe of the right hemisphere of the brain, whose vessels were nearly bloodless, all the other vessels, large and small, which we could see, contained bubbles of air, alternating with globules of blood, giving to them a curious beaded appearance. This could not have been the result of injury in removing the calvarium, for then only the larger vessels near the track of the saw would have been affected, whereas the smaller ones within the division of the hemispheres exhibited the same appearance. Nor could it have been from ordinary decomposition, for no other part of the body showed the least sign of decay, and the brain is far from being the first to undergo this change. Did it, then, take place during life, or after death? In speaking of similar cases, Vogel is of the latter opinion, for he observes, that “vesicles of air in the vessels of the arachnoid, if they actually existed during life and whilst the circulation was yet proceeding, would, in accordance with the ordinary laws of physics, be conveyed with the blood to the heart”^b. I confess myself quite unable to offer any explanation of the cause or the period of occurrence of this phenomenon.

3. *The State of the Kidneys*.—It will be remembered that we found them very highly congested, one of them very much enlarged; when divided, their substance was dripping with blood, and purulent matter escaped from their tubes, but was not collected in either large or small abscesses. What is the effect of inflammation and suppuration of the kidneys? Rokitanski, in his elaborate chapter on this subject, remarks that one or both kidneys may be affected: “In the latter case, especially, it is liable to terminate fatally, in consequence of paralysis of the renal function with typhoid symptoms, resulting from the detention of urea in the blood: this is frequently complicated with serous effusion into the ventricles of the brain, or into the pulmonary tissue, followed by putrescence; or if the inflammation reaches a certain degree of intensity, suppuration, or

^a Rowland on Softening of the Brain, p. 112.

^b Pathological Anatomy, p. 31, *Trans.*

an excessive retrograde process, or atrophy, may result; or lastly, the affection may become chronic"^a. Yet in the case before us we see that inflammation and suppuration existed not only without giving rise to these effects, for there were neither typhoid symptoms, paralysis of the renal function, effusion, nor putrescence, but with symptoms of quite a different character.

Dr. Simpson has published some cases of puerperal nephritis, which are more to our purpose, as showing a connexion between that disease and convulsions. I shall give an extract from one:—"The lady had so perfectly recovered, after a labour which was quite natural, as to have been out at church, &c. Seven weeks, however, after delivery, after some sudden and anomalous affections of sight and hearing, for thirty or forty hours previously, she was seized with most severe convulsions. Despite of free evacuations, &c., they continued to recur from time to time, and proved fatal in three hours; the patient, during that time, never being perfectly sensible. The pelvis of each kidney was filled with a whitish, purulent-like matter, and its mucous lining membrane coated with large patches of adherent coagulable lymph, or false membrane; the ventricles of the brain were distended with serous fluid; the urine, when tested, presented no trace of albumen"^b. Here, then, we have inflammation and suppuration of the kidneys giving rise to convulsions; and we have seen that the step from convulsions to paralysis is but a short one, for we have seen them co-existent, and both, probably, owing to the same cause, viz., some noxious element in the blood. It strikes me, therefore, as possible that the disease of the kidneys may have been the first morbid process in this case. Then if, as Mr. Henry Lee and others believe, pus in the blood disposes it to coagulate, and there is no fixed rule where this coagulation shall take place, or any relation between its origin and this local effect, it may be that coagulation, in consequence of the absorption of pus from the kidneys, took place in some of the minute vessels of the brain, causing obstruction and atrophy, or softening. But I merely throw out this as a possible explanation, without wishing to lay any stress on a mere hypothesis.

4. *The sudden Death*.—We have seen, not only that the life of the patient was for weeks preserved with this amount of disease going on, but that her condition gradually improved. Her intellect was as perfect as ever; her appetite was restored; and she could raise herself in bed and feed herself, with her

^a Pathological Anatomy, vol. ii. p. 191. Sydenham Society's Edition.

^b Edinburgh Monthly Journal, Sept., 1847, p. 213.

left hand, of course: at this period, after a fit of hearty laughter, she fell back and died. What was the immediate cause of death? for nothing was discovered to explain it. There was neither hemorrhage, nor rupture of any organ, nor clot in the heart, &c. &c.

We know from the experiments of Bichat, Magendie, Piedagnel, and others, that air injected into the veins proves suddenly fatal; and also, that in surgical operations, sudden death has resulted from the ingress of air into the veins, as was observed by Beauchene, Dupuytren, Mott, Cooper, Warren, &c.; and Dr. Cormack has suggested this occurrence as an explanation of the sudden deaths which occur after delivery. But in the present case we have no evidence of the entry of air, and no obvious place at which it could enter; for the uterus, as we found, had recovered its natural size and condition. Or if the air in the vessels of the brain be taken as evidence, either of its entrance *ab extra*, or of decomposition, can we possibly assume that there was sufficient to paralyze the action of the heart. Without offering any positive explanation, I cannot but think that the termination of this case resembles more those cases of fatal syncope which occur after delivery; or that state which Mr. Chevallier termed "idiopathic asphyxia," than any other with which I am acquainted. In those cases the patient makes some sudden exertion, sits up, or rises from bed, &c., and falls back lifeless or dying: on examination, nothing is found to explain this sudden termination. So in our case, whilst there was disease sufficient to extinguish life, there was nothing discovered to account for the sudden death.

ART. XII.—*On Dislocation forwards of the Upper Extremity of the Radius.* By WILLIAM TAGERT, Senior Surgeon to Mercer's Hospital.

DISLOCATION forwards of the upper extremity of the radius is an accident of rare occurrence, often difficult to diagnose, and sometimes foiling the best-directed efforts to reduce it even when recent, Cooper, Cline, and other able surgeons, having failed in their attempts; and in some instances where reduction has been accomplished, it has been found impracticable to prevent a recurrence of the displacement; and further, there are so many points in relation to this injury upon which practical surgeons differ, and which can only be cleared up by an accumulated record of cases, that I am induced to give an example of a dislocation forwards of the head of this bone, lately un-

der my care in Mercer's Hospital, which was successfully and permanently reduced.

John Gahan, aged thirty-eight years, was admitted into hospital the 21st of November, 1853, having sustained an injury of the left elbow-joint a few hours previously. He states, that while driving a hackney car, with a passenger and luggage, to one of the railways, his left arm was placed inside a strap, which secured a heavy trunk that he was trying to keep steady; the trunk tilted from its place, dragging him from his seat, and, while endeavouring to save himself from falling, his arm was violently wrenched and he suffered extreme pain. On the evening of his admission, powerful, persistent, but ineffectual efforts were made to reduce it by forcibly bending the forearm over the knee placed in the flexure of the joint; and also by strong and continued extension by traction from the hand, and concentrated pressure upon the head of the radius.

He came under my charge on the following morning. I found his forearm slightly flexed and pronated; I could with difficulty bend it to a right angle; further flexure was impossible, a sudden check being given in the attempt. I could not straighten it fully; the efforts to extend it caused him much pain; the head of the radius was absent from its natural position beneath the outer condyle of the humerus; on making pressure at this point, the thumb sank into a yielding hollow; the motions of pronation and supination could be performed for him; the latter movement was somewhat restricted, and increased his pain; the head of the radius could not be felt in its new position, its depth, and thick coverings in a well-developed muscular arm obscuring it. The natural configuration of the limb was not altered except from some slight swelling.

With the judicious and effectual aid of my able colleague, Mr. Butcher, the bone was replaced in the following manner:—The patient was placed in a sitting posture, the trunk being fixed. A handle of a sweeping brush, with a towel wrapped round its centre, was placed in the flexure of the forearm, and maintained in this position by grasping it firmly above and below the elbow joint. Mr. Butcher with one hand seized hold of that of the patient, and bent the forearm round the brush handle, which was forcibly pulled in an opposite direction, while with the thumb of the other hand he pressed firmly on the upper end of the radius, pushing it towards its natural position, at the same time supinating the forearm and abducting the hand; after employing considerable force in these movements, the bone was reduced, with a prolonged creaking or tearing sound audible by the class of pupils. The man was

conscious of the reduction, exclaiming that it was in, and expressed himself as at once relieved from pain. The forearm could be fully bent, and the patient's hand placed upon his left shoulder; the head of the radius could now be felt in its natural position.

The forearm was next flexed to a right angle, and midway between pronation and supination; an angular splint, well padded, was placed on the inner aspect of the arm and forearm, and firmly secured by carefully applied bandages; a sling completed the apparatus; the splint was kept on for a period of six weeks, rarely requiring readjustment; on removing the retentive apparatus, the functions of the arm were perfect, and the patient pursued his ordinary avocation.

Much difference of opinion exists amongst authors and practical surgeons, in reference to the comparative frequency of the displacement of the head of the radius backwards or forwards, and some with considerable experience have never met with either accident.

Boyer doubts that the luxation forward can occur. Sanson denies its possibility. Mr. Adams, of this city, a distinguished surgeon and pathologist, states in a paper on the "Abnormal Condition of the Elbow Joint," in the *Cyclopædia of Anatomy and Physiology*, that the luxation backwards is the most frequent the upper extremity of the radius is liable to, and considers that there are physiological grounds to sustain his opinion; he remarks "that the joint is less sustained behind by muscles than in front; that there is more latitude given to pronation, and that the pronators are powerful muscles; that during forced pronation, the upper end of the radius has a strong tendency to pass behind the axis of the humerus; and further, that supination is not so frequent, the muscles that effect it not so powerful, and the oblique and interosseous ligaments which do not restrain pronation are rendered tense and oppose a forced supination, which is the movement most likely to be followed by luxation forwards."

Bransby Cooper, in his *Lectures on Surgery*, agrees in opinion with Mr. Adams, and accounts for the greater frequency of the displacement backwards from the greater extent of motion in pronation, and says in addition, that "the anterior edge of the lesser sigmoid cavity of the ulna projects considerably more than the posterior, which offers less resistance to the displacement of the head of the radius in the latter direction." These may appear convincing arguments to show, that the dislocation backwards *ought* to be more frequent than that forward, but that it is so, is very far from being proved. Thus we find,

that Sir A. Cooper, on the opposite side, tells us that he has not seen an example in the living body of the dislocation backward, but gives an account of one which had never been reduced that was found in a dead body; while, on the other hand, he gives us six examples of the dislocation forward which he witnessed.

Fergusson, an eminently practical writer, tells us in his *Surgery*, "that from the frequency with which the luxation of the head of the bone forward is met with in the dissecting room, I am inclined to think that the accident is often overlooked, and I believe it to be much more common than that backwards." Erichsen, a late writer, in his work, "*The Science and Art of Surgery*," says, the dislocation forward is certainly the most common, and that he has seen many instances of it.

A diversity of opinion likewise exists as to the position of the forearm in the dislocation forwards; most surgeons regard it as in a state of pronation. Bransby Cooper says, it is permanently supinated, and others (Erichsen), that it is in a mid state between pronation and supination. Erichsen further states that, "the whole of the outer side of the arm is deformed, being carried somewhat upwards." He gives a woodcut of the deformity; such did not exist in the case I have related, nor is it mentioned in any author that I am aware of.

One point more of disagreement may be noticed, which refers to the mode of reduction, in making the extension for that purpose. Sir A. Cooper advises that supination should be performed, "as this position draws the head of the radius from the upper part of the coronoid process of the ulna, upon which it would otherwise be directed." B. Cooper tells us to produce forcible pronation of the hand, "which tends to direct the head of that bone back to its natural cavity."

These few remarks are sufficient to show the confused, inaccurate, and conflicting opinions entertained upon the dislocations of the head of the radius; and whether we regard the question of its displacements above, or below at the wrist, it may be well called a "bone of contention."

ART. XIII.—*On Treatment as a Test of certain Pathological Conditions.* By DANIEL GRIFFIN, M.D., M.R.I.A., Licentiate of the King and Queen's College of Physicians in Ireland, and one of the Physicians to the County of Limerick Infirmary, &c.

I BELIEVE the practice of testing the existence and nature of pathological conditions by the effect of treatment upon them is of extensive use in the profession, though no one, so far as I am aware, has made it the subject of a particular essay, or ventured to give to it that development which I think its importance so well deserves. I am satisfied there is no man of experience who has not been obliged, on many occasions, to resort to its guidance, and some of the most skilful and judicious have referred to it as affording the only solution of their difficulties in circumstances often of a serious and alarming character. Medical writers do indeed very generally attempt to draw a distinction between the symptoms presented by different pathological states which resemble each other; but wherever this attempt is made, it will be observed that such distinctions are entirely too fine-drawn for practice; the differences are so slight and unessential, that, as a basis for treatment, no confident reliance can be placed upon them. Thus, to take one instance, Mr. Langston Parker, in his excellent treatise on "*The Stomach in its Morbid States*," after stating these difficulties strongly, gives^a a table from Jolly, exhibiting the differences in the symptoms presented by nervous and inflammatory affections of the stomach, of which it may be said, that, while the distinctions are not by any means strongly marked, nor perhaps always well-founded in nature, they are in many instances entirely overthrown by several exceptional notes which Mr. Parker has placed, as the result of his own experience, at the foot of each page, showing, that so far from being well marked, or generally admitted, they formed matter for disagreement even between two pathologists of experience, who had made them the subject of their particular study. Indeed, Mr. Parker admits this fully a little further on, for he says^b, "*The symptoms of vascular and nervous irritation of the stomach are sometimes so similar that the most experienced practitioner in diseases of this kind is occasionally at a loss to decide upon their precise pathological character.*" I attended a patient for two years with all the symptoms of chronic gas-

^a Page 73.

^b Page 76.

tritis, which were accompanied by progressive emaciation. The peculiarity of the lancinating pains which were occasionally felt in the epigastrium, and the clean state of the tongue, made me suspect some cancerous affection of the stomach. The extreme restlessness of his nights led to the administration of opiates in the evening, after the disease had continued for eighteen months, when he was become much emaciated. From this time the signs of gastric disease began to decline, and at the end of some months, after having continued the opiates for the whole time, the patient had gained much strength, and was actually become fat. This, then, was evidently a disease of sensibility presenting all the symptoms of a chronic inflammatory state. . . . These nervous affections may consist sometimes in excitement (erethism), at others in diminished tone (atony), of the nervous influence of the stomach. They resemble, however, sometimes so closely the inflammatory affections, that the results of treatment are, occasionally, our only guides as to their nature." Again, towards the close of the work^a, he says, "There are many forms of gastric disturbance which resemble hyperemia, or inflammation, which are not benefited, but rather rendered worse by a treatment framed to suit such forms of disease; hence it is that we very commonly see local depletion from the epigastrium, and aperient medicines injurious to many affections of the stomach, which we should have supposed, from the symptoms which they exhibited, would have been benefited by such remedies. It is because these symptoms depend upon some other cause, and not upon hyperemia, or inflammation, that these remedies are not successful; and yet it is difficult, nay, sometimes impossible, to distinguish between vascular irritations of the stomach, and other affections of this organ, the results of treatment being occasionally our only guides."

Quotations of this kind might be multiplied far beyond the space I can afford, and indeed must be so familiar to those conversant with medical literature that anything beyond a general allusion to them may seem superfluous. I shall, therefore, add only one more, taken from the very excellent and practical Report on the Epidemic Dysentery of the North of Ireland, which was published in this Journal by Dr. Malcolm, in the Number for August, 1853. In that Report it will be seen, that, while the necessity for this therapeutic test is rationally and strongly insisted on, yet, in the attempt to discriminate the irritative and catarrhal dysentery from that which is purely

inflammatory, it is in some degree open to the remark I have made, in considering these cases generally, that the distinctions are, for the most part, too fine, for while any one or two of the distinctive symptoms may exist alone in any particular case, it is only the assemblage of a number of them that can form a basis on which we can rely with confidence in our treatment. Indeed, this admission is made in the observations with which it concludes, for a caution is very wisely enforced against adopting measures of too decided a character, even when the probable existence of a certain pathological state is pretty clearly made out during the progress of the disease. I do not, however, by these or any other remarks, wish to deprecate any rational attempt at these distinctions; they are not only essential to the scientific treatment of disease, but they must precede every plan of treatment. Disease can only be treated properly by a reference to the conditions that are present, and these again can only be made out in the first instance from the symptoms. A dash into treatment, therefore, without the fullest consideration of them, would be the merest empiricism. I only mean to draw attention to the fact, that, from a singular law in the system, very opposite states will exhibit themselves in certain organs merely by an interruption or derangement of function, and, therefore, present the same symptom. This fact explains why the symptoms become, in many cases, such uncertain guides, and why the final appeal in such circumstances must be to the effect of treatment. The passage in the Report is as follows:—

“It appears that while a considerable variety of forms obtained in this epidemic, the prevailing one was decidedly the inflammatory. This is, we think, deducible, not merely from the opinions of the reporters, but also from the comparatively superior efficacy of the antiphlogistic plan of treatment; hence the value of early discrimination of this form. It is quite true that the opium system, or a few doses of opium with mercury, or even a completely expectant mode of management, had its quota of recoveries. Such facts show that dysentery may be merely catarrhal, or the result of purely accidental irritation; but in a given case, should this plan fail, there cannot be a doubt that we should be prepared for meeting with promptitude a serious and extensive inflammatory lesion—always, of course, excepting those rare adynamic forms dependent, chiefly, upon the agency of an animal poison. Now, the nice point is, how are we to discriminate the presence of this inflammatory form sufficiently early? In many cases, no doubt, there will be little difficulty—the pyrexia being marked, and the local

symptoms prominent. But it is not always so: treacherous cases are not unfrequent which deceive even the most experienced. It is important, then, to have some resource to which we may have recourse in such a perplexity; and, fortunately, we may rely in a great measure upon the effects of remedies. If these be inefficient in cases seemingly of the mild forms, or give not satisfactory results within a very short period, let us at once look upon the case as one tending to present the higher grades of the inflammatory process, and act accordingly. Of course, while suggesting this counsel, we do not for a moment wish it to be understood that the antiphlogistic treatment is to be at once adopted in its entirety and purity,—such would be irrational,—but merely that it is to form the guiding idea in our management, permitting of such modifications as the particular circumstances of individual cases will always indicate. Besides this therapeutic test, we have every reason to believe that a closer examination of the patient than what is usually adopted may furnish sufficient data for early diagnosis of the existence of inflammation, more especially the examination of the abdomen by *palpation*, the changing of the tongue just at the lip and edges, the creeping up and alteration of force in the pulse, the temperature, the state of the urine, and the appearance and persistence of tenacious and coloured discharge (like pneumonic sputa)."

Perhaps one of the reasons why this appeal to treatment is less insisted on as a principle in medical writings is, that such a mode of practice has on the face of it rather an empirical appearance. The trial of one mode of treatment, because another has failed, must often appear but the merest experiment, and few people are willing to confess that their practice is often experimental; this, however, is not a correct idea of the practice. We try one mode of treatment, not, simply, because another has failed, but because in failing it revealed the existence of conditions, which, if we had been aware of their presence at the outset, would have rendered our treatment from the first what we are now making it. Another reason is this: some physicians are gifted naturally with a singular tact in discriminating the real conditions of disease;—it is a faculty, indeed, of the highest value in practice,—one which all who possess it in any considerable degree feel a very excusable pride in exercising and exhibiting:—therefore, a mode of practice which seems to ignore this fine faculty, and would be represented as appealing to experiment, as it were, in preference to it, can never be popular. Nevertheless, it must be remembered, that the exercise of this talent is itself far from

being free from the danger of serious and irretrievable error, and that the mode I speak of, though it may occasionally involve what seems an appeal to experiment, is, for the most part, as I have said, a mere adaptation of the treatment to conditions newly disclosed, the existence of which we had before no positive evidence of, and which were only revealed by the result of the previous treatment. Moreover, its value is principally perceived on those occasions in which all our previous means of discrimination, of whatever kind, have been exhausted.

The cases in which it is applicable are numerous, and have a considerable variety of character, but I shall confine the present paper rather to an illustration of the subject by a few examples selected from different classes, than by a more ample and perhaps tedious detail. It will be seen that, in certain circumstances, it is by it alone we can obtain reliable evidence whether an existing inflammation is acute or chronic, whether typhoid symptoms are dependent upon direct debility or upon an attack of inflammation of an intense character—whether symptoms of chronic inflammation depend upon the actual existence of that state or mere functional derangement, as well as several other important conclusions. In reasoning on the subject, however, considerable caution is necessary. All the points in the history of each case must be weighed exactly, and a due degree of importance attached to them, and no more.

And this is the proper place to remark, that by the past treatment I mean any treatment that may have existed, or no medical treatment at all,—for it is an important fact that in some circumstances a merely expectant treatment may be of a very positive character as regards the existing disease: as, for instance, in cases of obscure inflammation associated with symptoms of debility, of which an example will be given; and some others also, of which there is a considerable variety.

CASE I.—*Bronchitis accompanying Hooping-Cough.*—G. G., a healthy boy, aged fifteen months, after spending some months at the sea-side, and returning remarkably vigorous and robust, was seized with hooping-cough. The disease exhibited itself as hooping-cough often does at its outset, as a severe cold affecting the chest and air-passages. In this state, the cough being very moderate, he was taken out to drive on an unfavourable day, under the idea that the air would do him good. In a day or two, being worse, he was put under active treatment, was smartly purged and blistered, and took nitre and tartar emetic in large doses, which from time to time vomited him, and with apparent relief to the symptoms. The disease, how-

ever, went on insidiously—the fits of coughing were never severe nor violent; the attending fever was not very acute, nor the skin hot, and there was but little thirst, but the breathing by degrees became hurried, and though the blisters were repeated on different parts of the chest and back, and the other treatment persevered in, the bronchial affection became aggravated by degrees until at length it reached a condition that was quite alarming. About the end of the second week his state was as follows: face rather flushed, skin somewhat hot and moist; pulse 130, and small; much dyspnoea; breathing irregular, about 50 in a minute; humid rales all over the chest, but no dullness; expectoration abundant, but difficult, and of frothy mucus, the difficulty of breathing being aggravated when the fits of whooping came on, which, however, were neither frequent nor well marked. A consultation being held, it was a question whether the attack was still acute or had assumed a chronic form, and what degree of depletion, if any, the child would bear. He had been kept upon mild drinks—milk and water, and farinaceous food, of which indeed he took but little from the commencement, and, as almost the only thing which had been omitted was bleeding, it was determined to apply a small number of leeches to the chest, and watch the effect. Three leeches were applied, which bled very freely. There was immense prostration; the child grew pale; the pulse very feeble at 150; the breathing about 70 in a minute, the dyspnoea urgent; and the rattles in his chest almost suffocating. In point of fact he was in every respect vastly worse. We immediately gave him a small quantity of fowl broth every third hour, and a little wine alternately with it. Twelve hours after this plan was begun, the improvement was so decided and remarkable that it was surprising to witness it, and in the course of the next day he seemed placed almost in a state of safety. The shock of the disease was such, however, that though the improvement was uninterrupted, the process of reparation went on but slowly, and he retained a certain degree of delicacy for a considerable time afterwards. The mother of the child very naturally attributed the amendment to the leeches, but it was evident their application served but as a kind of *experimentum crucis*, revealing the real pathological state, and thus conveying a valuable indication.

It may be questioned whether the inflammation in this instance might not have been prevented from assuming a chronic character if depletion had been used in the commencement. From a circumstance which took place afterwards, I think this is at least doubtful. The same child, at eight years

of age, during an attack of measles of a mild type, was suddenly seized with laryngitis, from the extension of an inflammatory sore throat that attended the eruptive disease. I had been in his room a short time previously. He seemed to be going on well, but I was not many minutes down stairs when a fit of coughing seized him, and I heard the peculiar deep sound of laryngitis ringing through the house. When I ran up stairs again, I found a good deal of dyspnœa accompanying it. Eight leeches were immediately applied, which bled freely, and he was put upon frequent doses of calomel and James' powder, and blistered. The laryngeal affection subsided, but was followed in two or three hours by a good deal of prostration, and such an immense effusion of frothy mucus in the trachea and larger bronchial tubes, that I thought he would be suffocated. This recalled to my recollection the increase of the bronchial affection and the aggravation of all the symptoms which followed the leeching on a former occasion. The effusion disappeared by degrees under a mild supporting regimen, but it is probable an emetic or a blister alone without the leeching would in this instance have been a more appropriate remedy.

CASE II.—*Pneumonia with typhoid symptoms*.—W. C., aged 19, an attorney's clerk, occasionally intemperate, six days ill; skin cool, but dry; tongue loaded, brown and dry; countenance heavy; pulse 144, undulating, feeble, and intermittent; no petechiæ nor maculation of any kind; delirious and constantly muttering in his sleep, but when distinctly questioned answers rationally. Much thirst; some cough and short breathing, which last, however, is hardly more than proportioned to the rapidity of the circulation. Sputa muco-purulent and rust-coloured, but latterly less dark; left side dull over its whole extent, with some moist sounds, but no vesicular murmur. Right side dull at its lower third, with slight moist crepitation, and absence of the vesicular murmur. No treatment had been adopted, but he had plenty of gruel and whey.

Here was a case of a very alarming character. It was obvious an intense inflammation was present, but what was the true and entire pathological condition? Was the debility direct or indirect? Was it a case of typhus fever complicated with pneumonia? Or was it one of pure inflammation running its course without let or hindrance, and by its vehemence paralyzing the circulation, destroying the functions of the lungs, and, through them, those of the brain and other organs? The question was one of great practical importance, because, though depletion seemed essential in the circumstances, it was certain that any considerable amount of it would be borne badly if the

case should turn out one of pure typhus fever, and the state of the circulation was already very alarming. I know of no cases indeed in which the sense of responsibility descends with so much force upon the mind as in these. The circumstances call for a decision on the question, and, unlike those of a chronic character, this decision must be prompt and even immediate, or the time will pass when it can be of any service. An expectant treatment has been tried and results in the symptoms I have detailed. Moreover, when our decision is made, the measures must, in one case, be somewhat of a heroic kind, for any other will tell neither way and give no indication, yet the evidence as to whether the debility is direct, or the result of an intense inflammation is, to say the least, doubtful, and an error of opinion either way may be fatal. On reverting to the result of the past treatment as a guide, I reflected that, though it was merely expectant, yet it *had* a character. It was sufficiently supporting to prevent so alarming a degree of debility occurring in a simple fever of only six days' duration, while it was quite sufficient to feed an intense inflammation, and could not, in this respect, be considered at all strictly antiphlogistic. I therefore determined to assume that the pathological state was one of inflammation, though in some points expressed in terms of a typhoid character, and to act accordingly, to watch the effect, and press the treatment or desist according to circumstances. Eight good leeches were first applied to the left side, and a blister over the chest. He was put upon calomel and hydrargyrum cum creta, with small doses of Dover's and James' powders every third hour, and had a diaphoretic mixture of solution of acetate of ammonia with each powder. The following is the note in my case-book next morning:—

“The leeches continued bleeding all night, and are bleeding still. The cough is much less distressing, and the oppression abated; pulse substantial and regular at 124; tongue cleaner, and his general aspect is better. His bowels have been freely moved; discharge rather natural, but fetid. Kidneys act well; water depositing a copious sediment. Pergat.

“Sunday, 23rd.—He seems worse this morning: pulse 136, and very feeble; cough and dyspnœa not worse, but there is constant delirium. On inquiry I find he has had no gruel, and nothing but whey since my visit of yesterday. Continue the medicine and give two ounces of gruel every two hours, and whey *ad libitum*.

“Monday, Jan. 24th.—Much improved; cough easy and dyspnœa less; pulse steady at 118, and general appearance

good; skin soft and cool; bowels well moved; discharges still fetid."

The notes taken on the 23rd and 24th show how near this case was to the point of direct debility when depletion could be no longer borne. They also show the necessity of watching patients in this state lest their strength may run down for want of the due administration of their ordinary support. The late Dr. Graves, in his Clinical Lectures, makes some important remarks on the necessity of attention to diet in cases of fever, an attention that would be well bestowed in other cases also. Dr. Stokes makes a very interesting allusion to this trait of this late eminent physician in his discourse on his life and writings, read before the Association of the King and Queen's College of Physicians in Ireland on a late occasion.

In the further progress of this case, the symptoms declined by degrees; the respiration improved in both lungs, but more decidedly in the right; a considerable portion of the left, however, went through the suppurative stage. He had constant copious expectoration of purulent matter for many weeks; hectic fever; profuse night perspirations; great prostration of strength; and considerable emaciation. If the stethoscope still needed any testimony in its favour, I do not know when I met a case that proved its value more decidedly than this. It had every appearance of confirmed consumption, and by the symptoms alone, apart from any physical examination, could not in any way be distinguished from it; even a reference to its history and origin could not have been sufficient; but the stethoscope pointed out distinctly where the mischief lay, and showed the rest of the lungs, as the case advanced, so free from disease, that it could not be doubted these severe symptoms were the result of the third stage of inflammation. The young man made a perfect recovery, and is now at his usual occupation, in the enjoyment of excellent health.

CASE III.—*Functional Derangement simulating Chronic Gastritis*.—A lady, about thirty-seven years of age, a *religieuse*, and unmarried, was attacked with the usual symptoms of chronic gastritis in a mild form, but attended with the most overwhelming sense of despondency. Nothing could persuade her the attack would not end in dissolution. She had tenderness on pressure of the epigastrium, with occasional pain; the tongue was clean and small, but of rather a red colour; the bowels open, the discharges being yellowish and rather thin. There was a total loss of appetite, and but little desire for drink. She was leeches and blistered more than once, and under the use of

hydrargyrum cum cretâ and Dover's powder in small doses, with a mixture of infusion of cascarilla with hydrocyanic acid, sulphate and carbonate of magnesia, seemed to improve for a time. She was soon afterwards seized with vomiting, which nothing could allay, the fluid thrown up being acid. She got some mixture of chalk with hydrocyanic acid, with some relief again. This was succeeded by a diarrhœa, the discharges being thin, of the same colour as before, small in quantity, but hot, sharp, and irritating, and producing much tenesmus. The chalk mixture was persevered in, and the stomach again blistered, but with very little effect: indeed she appeared all the worse for it. The diarrhœa, in spite of opiates added to the mixture, went on, and the vomiting became so incessant that not even a teaspoonful of anything solid or liquid could be borne for a moment. Her despondency still continued, but she began to be reconciled to a result which she had considered from the first inevitable. Various means were used to control the symptoms, but without effect. After a day or two more she began, from pure exhaustion and want of nutrition, to show typhoid symptoms; the tongue became dark and dry, the pulse small at 130; there was some tendency to delirium, and I was apprehensive myself at last that her worst fears would be realized. I mention these particulars to show to what serious results, even in a very ordinary case, a want of sufficient attention to a single symptom, and that probably a mere effect of the diseased action, will lead. The acidity of the matter ejected from the stomach had continued from the first, and became now more intense than ever. I had been sensible of the importance of this symptom, and from past experience felt the necessity of correcting it, but imagined the antacids and absorbents already prescribed were sufficient for the purpose. It was clear, however, that unless some more effectual means were applied with that aim, the case would be lost. A draught was therefore ordered of fifteen grains of carbonate of soda with one-twelfth of a grain of acetate of morphia, and three drops of hydrocyanic acid, to be taken three times a day. It was surprising how completely this arrested the morbid action, and removed all the distressing symptoms. The diarrhœa, vomiting, and general irritability, all ceased within a day. She was soon able to take some food, and recovered completely and perfectly, without any change in the treatment except lengthening the interval between the doses of the alkali as the improvement went on. From the rapidity of her amendment, it was evident the affection was a merely functional one, and I have very little doubt that if the same had been prescribed at a sufficiently early pe-

riod, there would have been no necessity for either bleeding or blistering.

Some cases of chronic inflammation of the eye resemble those of the acute form so completely that it is wholly impossible to distinguish them except by the effect of treatment. In this, as in other cases of diseased action, I have seen practitioners of eminence try to decide between the two states by the character of the pain or its absence, by the presence or absence of heat and throbbing, the tears being hot or cold, and other particulars; but I am satisfied, from numerous cases that have fallen under my observation, that, whatever may be the result generally, there are several instances in which this cannot by any such means be done. Many of these, indeed most of them, are acute at first, bear depletion very well, and are much benefited by it, but, as time wears on, we find that a perseverance in the same means does not effect a cure, nay, that they are rendered stationary, or even made worse by it, and it is only on the repeated application of such means, and their being pressed to an extreme when they do not succeed at first, that we see they are ineffectual for the end proposed. Cases are met with from time to time that bear out these observations in various degrees. The following is one of the most pointed that has occurred to me.

CASE IV.—*Chronic Conjunctivitis*.—E. S., a stout young woman, about twenty-five years of age, and in good health, was admitted into the county of Limerick Infirmary, with ophthalmia of both eyes, the left being the worst. She had been ailing about three weeks, but employed hardly any remedies except some aperients and fomentations. She was bled and leeches freely, purged and blistered, and with considerable benefit, for a time; she then had a relapse, supposed to be from cold; the same means were repeated and renewed, and again with benefit, when another aggravation occurred. After this had taken place three or four times, the state of the eyes became worse than ever, and I began to suspect these repeated relapses were not wholly due to the cause assigned. All local applications were merely palliative; the inflammation seemed now extremely intense; there was great heat in the brows, headach, throbbing, and much intolerance of light; the conjunctiva was deeply bloodshot, and the tears hot and burning; there was, however, no chemosis, and no destructive process was going on; nor was there either in the girl's constitution, nor the aspect of the case generally, any sign of a strumous diathesis. On going over the history of the case with some pupils of the hospital who were present, I directed their

attention to the degree to which the antiphlogistic treatment had been repeatedly pushed, and its evident incompetency to effect a cure; and I felt so confident in my idea of the nature of the difficulty from this fact, that I ventured to predict that, severe as the symptoms now were, they would disappear under a more generous diet; I therefore ordered her meat and porter each day at dinner. The result was as I anticipated; the severity of the symptoms immediately abated, and she got perfectly well in a short time without any further interruption.

Inflammations of the eye have a peculiar interest for the pathologist, because the conditions of the inflammatory process, which in other organs are a good deal a matter of inference, are here rendered open to ocular inspection: but if this be true as regards inflammatory affections of the external parts of the organ, the interest is considerably enhanced when we turn to the same affections of its internal cavities, especially those parts of them which are most accessible, such as the iris, the posterior and anterior chambers, and the fine membrane by which they are lined. We have here, indeed, a type, or rather a beautiful miniature, of serous cavities in general, the same in structure, and subject to the same affections, the same laws, and the same results; and, when its parts become inflamed, we see without more ado the various stages of inflammation pass, as it were, in review before us,—the congestion, the capillaries carrying red blood, the effusion of purulent matter, or of turbid fluid, and coagulating lymph; the whole process, in fact, which in other serous cavities—such as those of the pericardium, the pleura, or the peritoneum—are only eliminated by a careful analysis of symptoms, and an anxious exercise and exaltation both of the intellect and the other senses: moreover, when remedies are applied, we very soon see whether they are beneficial or detrimental, and that by a mode beyond dispute; and the interest of the subject is still further augmented by the consideration that if any useful conclusions are deduced, they must be, from the laws of physiology, of very general, if not universal, application in similar structures.

These observations will explain why I attach more importance to such cases as I have given, and the effect of treatment upon them, than at a first glance they would seem to merit. It is true that acute and chronic inflammations often resemble each other very closely, while they require different modes of treatment; and it is also true that in several instances the distinction between them can be made by the effect of treatment alone. In all this there is nothing new; nothing that has not been discovered by every one who has been long in practice;

therefore, what I wish to insist on is not so much the facts themselves, though they are valuable, as the very general application of the conclusions which may be drawn from them. If it be true that certain inflammatory affections of the conjunctiva of the eye, or of the bronchio-pulmonary mucous membrane, are capable in some circumstances of being greatly benefited by an improved diet, the same will be true, also, though with some reservations, of similar affections of the mucous membrane of the stomach, of the whole tract of the alimentary canal, and of other mucous surfaces. Cases might be cited in proof of the correctness of this conclusion, but I conceive it is unnecessary: it receives a strong confirmation from the experience of many able physicians, among whom I may mention the late Dr. Graves, who says^a, "that in chronic dysentery, meat is far too much refrained from; and that many cases which obstinately resist the most varied remedies, assiduously employed, get well rapidly after a liberal allowance of meat is given to them." To the truth of this assertion Dr. Mayne also bears ample testimony in an interesting article on chronic dysentery, published in the *Dublin Journal of Medical Science* for November, 1850, "having often," as he says, "succeeded in this way after the failure of all other measures."

I have said, "with some reservations," for there is one important point to attend to in the application of this principle to diseases of certain organs. The advantage gained by an improved diet in chronic cases operates probably indirectly through the medium of the blood; and as the mucous membrane of the stomach and intestinal tube is itself intimately concerned in the process of assimilation, care should be taken not to administer nourishment in an unsuitable form. It must be as easy of assimilation, tender, and digestible as its nature will admit of, otherwise it will only add to the embarrassment already existing.

Having spoken of the mucous surfaces, I am anxious, even at the risk of being thought tedious, to illustrate certain affections of the serous cavities, usually originating in inflammation, by a reference to some interesting cases already alluded to; I mean cases of iritis. Many of these, even when not of a specific character, are certainly capable of being affected by specific remedies; and whether affections of the serous cavities in general are so or not, is, I think, a very interesting point for investigation. The embarrassment arising in affections of the internal parts of the eye resembles very much what has been

^a Clinical Lectures, edited by Neligan, vol. ii. p. 138.

already described as occurring in other tissues. They improve in the early stages under bleeding, general and local, and mercurialization; a relapse or two occurs, and they improve again; afterwards they remain stationary for a time, or fall off, and then the more these remedies are pressed the worse they get, until it is certain that a perseverance in them would operate to the total destruction of vision. It is at this point that, if the former treatment have not been pushed to a ruinous extent, I find the most singular and decisive effect from the use of oil of turpentine in doses of twenty or five-and-twenty drops three or four times a day. Its wonderful action on the absorbents will be best understood by citing a remarkable example.

CASE V.—*Iritis*.—H. M., a healthy young man, about twenty years of age, was admitted into the county of Limerick Infirmary for an attack of iritis. I could not discover that he ever had a venereal complaint. The iris was changed in colour from its natural gray to a greenish cast; there were several little buttons of lymph on its surface; some enlarged vessels carrying red blood were also visible, and it was almost immovable; the pupil was dull, the vision greatly impaired, and the usual characteristic zone of red vessels surrounded the cornea. He had been under my care before he came into hospital, and entered it at my request, as the treatment could not be efficiently carried out in the uncomfortable lodgings in which he was placed. He was bled and leeches repeatedly, and got under the influence of mercury, and with very considerable advantage for a time. After two or three relapses such as I have described, when the same treatment was renewed it was found ineffectual. As his health and strength seemed good, the antiphlogistic plan was pressed further, but was soon followed by a considerably increased effusion of lymph, which at last filled the whole anterior chamber, and blocked up every point through which light could enter. The pupil, however, could still be dimly seen, but it was irregular and quite motionless. I do not know when I saw a more unpromising case. In fact, the boy was nearly blind, and as it was clear to himself, as indeed to every body else, that he was in a much better condition when he first came into hospital, he requested to be allowed to go home. Feeling how discreditable such an event would be in the circumstances, I earnestly pressed him to remain a little longer, partly under the pretext of getting his sore mouth well. To this he at last reluctantly consented. All the antiphlogistic measures were immediately suspended. He got a lotion for his mouth, and was ordered twenty drops of oil of

turpentine three times a day. It was a week before the effect of this change was distinctly perceptible, but when once he became sensible of it himself he was very willing to go on. Although the alteration was slow, it was steady and uninterrupted. The effused lymph was by degrees taken up; the pupil regained its roundness and clearness, and the iris its motion; the process of reparation took several weeks, but his sight was nearly restored to its usual sharpness on leaving the hospital. No further change of treatment was necessary from the time the turpentine was commenced.

I have preferred giving a single striking case of this kind in detail to entering into general expressions as to the efficacy of the remedy, because I observe that, though its efficiency in these and other such instances is admitted by those who have tried it, it yet seems to be looked on as a mere specific in certain cases, and its power as a general agent in controlling inflammatory action and removing its consequences seems to be a good deal lost sight of. The same was the case with calomel. Its energetic employment in the diseases of liver so common in India made it at first looked on as a specific in that complaint, and it was a long time before it was perceived that its success in such cases arose from a power it possessed to control inflammation generally after proper depletion. That the same idea of specific action should still prevail with regard to turpentine is curious, for we find cases recorded in the journals which show its success in inflammatory affections occurring in various structures and with several varieties of character, the prevailing one being that in which other means had failed. Those I have seen myself, though none of them were so remarkable as the one I have quoted, have been certainly sufficiently numerous to establish the conclusion I have been aiming at, that it acts on a general principle. It is not easy to explain what the conditions are that are most favourable to its success. If not inflammatory in their intimate nature they are certainly so in their origin. They are probably of a chronic character rather than acute, yet this is not their sole distinctive mark, for in the instance I have given there was no change of regimen nor any alteration save the employment of the turpentine instead of the means previously used. I can best describe them, perhaps, by stating that, according to my experience, the remedy does not succeed so well, or indeed, I think, at all, in the early stages of inflammation, but precisely in the later, where bleeding and mercurialization have entirely failed. Neither is it easy to explain its mode of action as some have attempted to do; it is sufficient on this point to say that all the explanations I have seen are quite in-

consistent with facts that have come under my own observation. I am so strongly persuaded, however, both of its efficacy and the general nature of its action, that I think it well worthy of a more extensive trial than it has yet received in several embarrassing cases of inflammation of serous membranes, and indeed of other tissues also,—the time most favourable to its action being, as I have said, when they are running into a chronic condition, and when depletion and mercury do not seem, after a fair trial, to have succeeded. What an invaluable addition it would be to our list of therapeutics if it was found capable of succeeding in certain unmanageable cases of chronic pleurisy, or still more, in those effusions of lymph and other structural changes that lead to alteration of the orifices, lesions of the valves, and other such ruinous results, in chronic cases of pericarditis and endocarditis^a.

In watching the effect of treatment as a guide to pathological conditions, we must be careful not to be led away too hastily from conclusions that appear rational, by any anomalous or unexpected results that arise during the application of the test. If our reasoning seems sound and good, and the anomaly is capable of being explained by a reference to other well-known pathological principles, we must persevere in our course, and devise some test applicable to the principle we have made use of to explain the exception. My meaning will be better understood by giving a case in point.

CASE VI.—*Epileptic Fits during Pregnancy*.—A young lady newly married, and in her first pregnancy, became subject to fits, which were called “weaknesses” or “fainting fits,” but which, on a more particular inquiry, I found to be of an epileptic character. They occurred about once in a month or six weeks at first, but towards the latter part of her pregnancy much oftener. They sometimes occurred when in bed, but more generally when she was in a stooping posture, and were very sudden in their access: when she came out of them her face was flushed and heavy, she breathed stertorously, and even when recovered spoke incoherently for a time. I saw her on one oc-

^a Since the above was written I have met with the following passage in Watson's *Lectures on the Principles and Practice of Physic*, p. 286:—

“But when inflammation has stiffened the valves of the heart, or studded them with little wart-like masses of fibrine, how far do these morbid states admit of perfect recovery? It is not so easy to say. I am not aware of any facts which would forbid altogether the hope that here, as in iritis, the re-absorption or removal of the lymph may be total, and the restoration of the parts complete; on the contrary, *the comparative* infrequency of wart-like excrescences in the slowly fatal cases of rheumatic carditis leads to the opinion that such deposits may disappear as readily and entirely from the valves of the heart as from the iris.”

casion directly after the fit. Her tongue was bitten severely, a result which had occurred in one or two instances before. Her pulse was, however, weak and languid, and on all these occasions beat from 70 to 75. When these fits first appeared, she tried many of the remedies usually prescribed in such cases, but every attempt to relieve her in this way was frustrated by the extreme irritability of her stomach, which rejected almost everything of the nature of medicine. As she had never been subject to these attacks before, I looked on them as associated with the pregnancy, and thought it likely from the stertor and other symptoms that they were accompanied or caused by local congestion in some part of the cerebro-spinal axis. To this opinion I was also led by her mode of life, which, though very regular, was well calculated to increase the plethora arising from her condition. She had a good appetite, lived well, dined late, and ate heartily. As her face was usually pale, the surface rather cold, and the pulse weak and languid, I had a great objection to resort to venesection without necessity; I therefore recommended her to take her principal meal in the middle of the day, to live rather sparingly, and avoid large quantities of liquids, continuing her usual exercise. As time wore on, however, when the attacks became more frequent I began to be apprehensive of puerperal convulsions, or some other dangerous consequence during her confinement, and advised a moderate bleeding from the arm, to which she consented. About sixteen ounces of blood were taken away. It was neither bled nor cupped, produced no faintness, and she felt very comfortable and pleasant during the whole evening afterwards. That night, however, about 12 or 1 o'clock, she had far a more violent attack than she ever had before. Her tongue was wounded more severely; the stertorous breathing was deeper, louder, and more prolonged, and the incoherence that followed lasted much longer than usual. I was, I confess, surprised at this result, but as I could hardly persuade myself my reasoning was false as to the tendency of her regimen to produce plethora, local or general, I attributed it to some peculiar effect on her nervous system before the circulation had time fully to restore its balance, and I was far from being satisfied that it proved the non-existence of the state I had assumed. In this idea I was somewhat supported by remembering the well-known pathological fact, that a single general bleeding, unassisted by local depletion is seldom capable of reaching or controlling an active local inflammation. I determined, therefore, to test the matter further by reference to this principle, and ordered leeches behind her ears, which, after a second application of them, in four

or five days produced so decidedly beneficial an effect that all tendency to a recurrence of the fit entirely ceased. She passed through her confinement without a single bad symptom, nursed her infant, and has enjoyed very good health ever since.

I must here bring this paper to a close. If circumstances permit, I may perhaps at some future time offer some further illustrations of the important subject of which it treats.

ART. XIV.—*On Dislocation of the Metatarsal Bones downwards and backwards*. By JOSIAH SMYLY, A.B., F.R.C.S.I., Surgeon to the Meath Hospital and County of Dublin Infirmary, &c.

IN the last Number of this Journal Mr. Tufnell has called the attention of the profession to “a form of dislocation of the foot not previously described.” He very properly says, “I would by no means infer that it is the only case that has occurred, for, happening under the agency of causes which must ever have existed equally as at present, we cannot but reasonably suppose that such accidents must have befallen individuals then as now, and that the want of a careful record by surgeons of their several experience has alone prevented this peculiar dislocation from being known.”

Mr. Tufnell's case is certainly the first upon record of luxation of the metatarsus *downwards*. A case of this kind came under my care in the Meath Hospital eleven years ago, which I ought then to have made known, as upon investigating the subject I found the case to be “unique,” the best authorities being ignorant of the existence of such a lesion, and some denying the possibility of its occurrence. It may be of use to bring forward my case even now, as a contribution to our stock of knowledge. No doubt many valuable facts are lost from want of diligence in collecting them, as well as from ignorance of their value. Mr. Tufnell has shown a good example—worthy of imitation.

William P., a young man, was admitted into the Meath Hospital on the 23rd of March, 1843, for an injury of the foot, which he sustained the day before. He was driving a cart, sitting in front, when, in making way for another vehicle to pass, the wheel got into the gripe of the ditch at the roadside, and the cart on which he sat was upset. He fell so that his right foot got between the shaft and the bank of the ditch. The shaft crushed the heel against the toes, which were fixed by the bank. The patient suffered severely at the time, and

was quite disabled. His foot was pulled immediately after the accident, but, getting no relief, he was sent to hospital. On admission, the foot was so swollen that the nature of the accident could not be ascertained; when, by leeching and appropriate means, the ecchymosis was dispersed, the form of the injury became manifest from the projection of the tarsus, the hollow immediately in front of it, with the corresponding projection in the sole, and the shortening of the foot. On the sixth day after the accident efforts were made, by means of pulleys, to reduce the dislocation, which, with perseverance, proved perfectly successful. A piece of wood, in the form of a sandal, made to fit the sole of the foot, having a heel-piece of leather with a strap to cross the instep, retained the bones in their places. On the 16th of April this man was dismissed, being sufficiently recovered to use the foot.

In this, as in Mr. Tufnell's example, the luxation was caused by a force acting between the heel and the toes, pressing the parts together. This case shows that there is no physical barrier to reduction in such dislocations, and that an attempt ought to be made before condemning such lesions as irremediable, and abandoning them to nature.

The most important difference between Mr. Tufnell's and my case is, that reduction was accomplished in the latter; this I attribute to the circumstance that in mine the whole range of metatarsal bones was dislocated, and thus there was a more extensive laceration of the ligaments; and, secondly, the extending power could be more efficiently applied than where only three bones were displaced—the two which remained *in situ* impeding and resisting extension.

ART. XV.—*Clinical Reports, and Observations on Medical Cases.*

By J. T. BANKS, M.D., M.R.I.A., King's Professor of Physic; Physician to the Whitworth and Hardwicke Hospitals; Examiner in Medicine to the Queen's University in Ireland^a.

PNEUMOTHORAX WITH EFFUSION: RECOVERY.

JAMES SMITH, aged 30, a painter, was admitted into the Whitworth Hospital on the 29th of August, 1851. His father died of tubercular phthisis, at the age of 44, and a brother died

^a Read before the Association of the King and Queen's College of Physicians in Ireland.

of the same disease in early youth: his mother is still living, and is in good health. Smith states that he has always been temperate, and of regular habits, and that he enjoyed excellent health until April last, when for the first time he became affected with cough, which he attributes to exposure to cold consequent upon the nature of his employment. He never had at any period hemoptysis. The cough was at first dry, afterwards there was a frothy mucous expectoration, which subsequently became purulent. About two months before his application to be admitted into the hospital, in a violent paroxysm of coughing he felt as if something suddenly gave way in his chest, and at the same moment he heard a noise; his breathing instantly became short and extremely oppressed, and he suffered from pain of an agonizing character in the left side. He was constrained to sit upright at first, but he was soon able to lie on the affected side; an attempt to rest for a moment on the right side, or even on the back, gave rise to a feeling of impending suffocation. Cough, with abundant purulent expectoration, continued. Emaciation was progressive, and night sweats constant. He states that he felt his heart *suddenly* move from the left to the right side of the chest, and about the same time he could perceive a splashing sound on rapidly moving his body and also on turning in bed.

When received into hospital, he presented all the physical signs which characterize the presence of air and fluid in the pleura. The left side over a considerable extent was tympanitically resonant, the lower part of the same side being perfectly dull on percussion: the line of demarcation which marked the change was abrupt. Alteration of position had the effect of causing the tympanitic resonance and the dulness to change places. The respiratory murmur was absent over the whole extent of the left side. Bronchophony and bronchial respiration were audible at the root of the lung. Metallic tinkling and amphoric breathing were also present.

The Hippocratic succussion was to be heard not only on placing the ear on the chest and moving the body, but it was even audible at some distance from the patient's bed. He had acquired by practice the manner of shaking his chest so as to render the splashing sound exceedingly loud, so much so that a man at the opposite side of the ward declared that he could distinctly hear it. The side was dilated, and the intercostal spaces effaced. The impulse of the heart could be felt below the right mamma; the sounds were perfectly natural; the pulse was 84 in the morning, and 96 in the evening. The respiration of the right lung was puerile. He was emaciated, and there

existed distressing cough with purulent expectoration, chills towards evening, and perspiration confined to the upper part of the body. From the time of his admission he improved, with the exception of occasional intercurrent attacks of bronchitis of the right lung, when the habitual dyspnœa and the febrile symptoms were considerably aggravated.

It is needless to pursue the minute details of the case to the period of the patient's leaving the hospital in the month of June, 1852. It will be sufficient to state that long before his departure the signs of air and fluid in the cavity of the left pleura had ceased to exist. For a considerable time a small space at the summit of the chest continued tympanitically clear. Gradually the respiratory murmur, feeble and distant, however, was re-established; the cough and purulent expectoration ceased, the pulse came down almost to the standard of health; the breathing, though frequent, was no longer difficult, and he regained in a great measure his lost flesh. On leaving the hospital the heart had not returned to its place, and the whole extent of the left side was dull on percussion.

On being discharged from the hospital he resumed his ordinary mode of life, again engaging in the occupation of a painter. A year elapsed, according to his own statement, from the first change of position of the heart until its return to the left side of the chest.

I had from time to time opportunities of seeing the man after he passed from under my care, and last month he applied for advice at the Whitworth Hospital, complaining of the symptoms of slight bronchitis. I admitted him that I might carefully investigate his present state after so long a period from the occurrence of perforation of the pleura. He informs me that he has been able to work at his trade uninterruptedly since he left hospital. He has been and is able to walk well, and even to "trot," but when he runs rapidly his breathing becomes very short; he continues from habit to lie on the left side; though pale and rather delicate in appearance, he is in tolerably good condition. The eye can detect no difference between the right and left side of the chest; there is no curve of the spine; no depression of the shoulder, and the intercostal muscles of the left side move in respiration as those of the right. On measurement, it is found that the left side exceeds the right by half an inch. The percussion sound is comparatively dull both anteriorly and posteriorly on the left side; the respiratory murmur is audible everywhere on this side, but is much more feeble on this than on the other. The respiratory sound at the apex is perfectly natural, except that it is not so loud as that at

the summit of the right lung. The heart is now at the left of the sternum, but its apex beat is about an inch to the right of the point where it is felt in health.

The phenomena resulting from the presence of fluid and air in the cavity of the pleura have, from the most remote times, attracted much attention, but, nevertheless, little advance was made, until a comparatively recent date, towards a true appreciation of the causes and nature of the disease. Even the knowledge possessed by Bayle and Itard can only be considered as vague and unsatisfactory; and fruitful in results as were the labours of Laennec, much was left by him to be worked out by his followers, stimulated to further research by his great example. Louis, Andral, Stokes, Haughton, Saussier, and others, have, by their investigations, cleared away many obscurities, and rendered the history of pneumothorax complete.

The essay of Dr. H. M. Hughes on this subject is replete with instructive and highly suggestive matter; and he has recently added to the store of facts already accumulated by the publication, in Guy's Hospital Reports, of additional cases, some of which are confirmatory of views advocated in his original memoir.

Dr. Hughes has combated the doctrines of the French pathologists as to the rapidly and *necessarily* fatal consequences of the effusion of air and fluid into the pleura, and supports his opinions by accurately recorded cases. Louis has particularly dwelt on this point in the history of the disease. The duration of life, in his cases, varied from sixteen hours to thirty-six days; half of his patients did not live more than three days, and only one attained the maximum of thirty-six days. He admits, however, that the time intervening between the moment of perforation of the pleura and death may be much more protracted; and refers to a case observed in his ward in the Hotel-Dieu, in which eighteen months elapsed between the perforation and death; the notes of which were taken by Dr. Baron, Jun.; and to a no less remarkable case in the clinical wards of Chomel.

In Andral's cases the extreme limit was four months^a. Saussier says, "La mort a eu lieu dans tout les cas où le pneumothorax était dû à une perforation tuberculeuse."

^a That some of the most highly distinguished French physicians hold that pneumothorax, with effusion consequent upon tubercular perforation, is inevitably fatal, is proved by reference to their published opinions.

Valleix, in speaking of the prognosis in perforation of the pleura, thus disposes of the subject:—

“On a vu, en effet, que, dans les cas de perforation, la terminaison a toujours été fatale, et que ceux où l'on obtenu la guérison laissent, pour la plupart du mois, des doutes sur l'exactitude du diagnostic. Nous avons vu aussi que cet accident abrège généralement les jours des malades. Cependant, M. Hughes avance *qu'il est probable que*, dans beaucoup de cas de phthisie avancée le pneumothorax a plutôt prolongé qu'abrégé la vie des malades. Cette proposition me paraît avoir un caractère *evident d'exagération*.”

The experience of British physicians contrasts remarkably with that of our Continental brethren, for gloomy as are the records of the disease, we are not altogether precluded from reliance on the recuperative powers of nature, aided by appropriate treatment.

Dr. Haughton reports a case in which life was prolonged for a year and a half after the establishment of pneumothorax with effusion; and it is impossible to affirm, that the patient might not have permanently recovered, had he been placed in circumstances favourable to such a result.

In a case recorded by Dr. Barlow, there was an evident arrestment of the phthisis, and the patient lived for three years. Of the cases which Dr. Hughes has seen, or the histories of which he has examined, one lived twelve months, one thirteen months, and one three years; and in his recent paper in Guy's Hospital Reports, he states that he saw a case of Dr. Bird's, in which pneumothorax occurred on the 19th of November, 1844; and the subject of this lesion lived until the 24th of January, 1848.

I may refer to the case related by Laennec, in which pneumothorax lasted for six years, which may have resulted from a tubercular perforation, and not from an empyema opening into the lung, as supposed by those who cannot conceive the possibility of the prolongation of life under the former circumstances. Laennec, from his observations of the healing of tubercular cavities, admits the *possibility* of recovering after tubercular perforation.

The suspension of the phthisical symptoms on the invasion of pneumothorax, and the consequent prolongation of life in some cases, have not escaped the notice of Dr. Stokes and the late Dr. Haughton. In his singularly able treatise on pneumothorax, in the Cyclopædia of Practical Medicine, Dr. Haughton ventured to suggest the “possibility of recovery,”

and sustained the supposition by facts and arguments, derived from the observation of the case alluded to; but adds—"We by no means wish to inculcate it in a positive manner; before this can be done by any one, a more enlarged experience must be possessed; and the observer who is desirous of deciding the question by facts, has to set out with the melancholy knowledge, that not one case proved by auscultation to be of this nature is on record, in which ultimate recovery took place."

In his recent observations on the subject of pneumothorax, Dr. Hughes goes beyond the mere prolongation of life in cases of phthisis, in which pneumothorax supervenes—the statement which calls forth M. Valleix's criticism, for he says;—"I am still clearly of opinion, that in some cases of phthisis, the occurrence of pneumothorax has tended to the prolongation of life; and that sufficient evidence will, I trust, be found to exist in the case last related in the present paper, to show that pneumothorax is not only not necessarily unsusceptible of cure, but that it has been actually cured, even when associated with phthisis."

The case which I have detailed is highly important, as well calculated to strengthen our faith in the reparative powers of nature in a disease by many believed to be necessarily fatal. I have had no means of arriving at a knowledge of the actual state of my patient's lungs, antecedent to the perforation of the pleura, inasmuch as the event had taken place before I had an opportunity of examining him; but from the clear and satisfactory account he gives of his symptoms, and the general state of his health, as well as from the hereditary tendency to strumous disease, indicated by his father and brother having died of phthisis, I have no hesitation in coming to the conclusion, that the pneumothorax was secondary to tuberculosis of the left lung.

It appears to me that in this instance there has been an arrest of the morbid action, and from the perfect freedom from physical signs and general symptoms of phthisis, now at the expiration of nearly three years from the invasion of pneumothorax, I see no grounds for being sceptical as to the recovery being permanent.

Of the possibility of the event, there can be no question when we contemplate the proofs adduced by Dr. Hughes, of pneumothorax the consequence of tubercular perforation being susceptible of cure, positive demonstration being afforded in one case of the healing of the pleural aperture, although the supervention of tubercular disease in the opposite lung induced a fatal termination.

In the majority of the cases of perforation of the pleura, the subjects of the lesion have been conscious at the moment of something having given way in the chest, and have experienced acute pain, accompanied by inexpressible anxiety, and extreme difficulty of breathing: so it was with the man whose case I have reported. But, in addition, he heard a noise, which he says came from his chest. I do not mean to deny that there are instances in which the moment of the perforation is undistinguished by any very marked symptoms, but most assuredly these cases are purely exceptional. That the phenomenon to which I have alluded, viz., the crack or sound of the rending of the pleura, has not attracted much attention, I assume from the fact of the infrequency of any mention of it in the published cases of the disease. I only know of reference to it in two cases, one by Louis, and the other by Dr. Stokes. My friend, Dr. Henry Thompson, has lately favoured me with the facts of a case of pneumothorax of great interest in a twofold point of view. A man was admitted into the Omagh Infirmary, with unequivocal signs of tuberculous cavities in the right lung, the left being sound. While in the hospital he was suddenly seized with intense pain, and a sensation of something giving way in the right side, and at the same time he heard a noise, which was also audible to the nurse, who happened to be near. The sound was compared by both to the cracking of an unseasoned table. Dr. Thompson informs me that this man left the Omagh Infirmary, after a protracted residence there, free from all signs and symptoms of phthisis, and having regained flesh and strength. A year after his leaving the hospital, Dr. Thompson heard from him, when he was in good health, but being in a distant part of the south of Ireland, he has since lost sight of him. There can be no doubt of this case being an example of arrested phthisis.

Pathologists have sought to explain why in one case death so suddenly follows perforation of the pleura, while in others life may be prolonged to a comparatively remote period. Louis has not come to any satisfactory conclusion. The opening of the pleura being valvular, and admitting the air freely, while its exit is effectually prevented, appears to be the most reasonable explanation of the rapidly fatal cases, while, on the other hand, a free communication, which permits the air to escape from the cavity, favours the prolongation of life. This is the explanation which Dr. Stokes has given, and which, doubtless, is satisfactory as regards prolongation of life to a certain extent, but in those cases which may be said to have ended in recovery, we must admit that in addition, the tubercular disease is confined to one lung, and, also, that probably the affected lung is not ex-

tensively tuberculated. We know that a single tubercle, situated near the surface, may, by bursting, occasion the effusion of air, the lungs, with this exception, being free from all trace of disease. The case I have reported may, I apprehend, be looked upon as one of those presenting all the conditions necessary to give a fair chance of restoration to health.

A brief notice of the treatment employed in the case will suffice.

After the acute symptoms which supervened on the perforation of the pleura had passed away, I proceeded to treat the case as one of ordinary phthisis, administering the remedy which has been well termed "analeptic" in its action on the system, viz.,—cod-liver oil. For six months steadily and uninterruptedly he persisted in the use of the oil three times daily. His appetite was good throughout, and he never suffered in the least from his stomach rejecting or loathing the medicine. For some time he took syrup of the iodide of iron, in combination with the oil. During the whole course of the disease his diet was of a highly nutritious character.

It would be unjustifiable in me to occupy space by accumulating evidence as to the power of cod-liver oil in phthisis—the experience of every practical physician will supply more or less of materials corroborative of its efficacy.

For the introduction of the use of this medicinal agent in the treatment of phthisis pulmonalis into this country, and for his carefully conducted researches on the subject of the arrestment of this destructive malady through its means, the profession is largely indebted to Professor Bennet, of Edinburgh.

In concluding the sketch of this case, and the observations suggested by it, I would direct attention to a practical inference which may be drawn from considering the present state of the patient.

In the diagnosis of any disease affecting his left lung, how many difficulties would beset the physician who might happen to see him for the first time. Universal comparative percussion dulness of one side (caused, I believe, in part by abundant deposition of lymph on the pleura), and a feeble respiratory murmur, constitute a combination of signs eminently calculated to mislead and embarrass the most accomplished auscultator.

[After remaining for a few days in the hospital, on the occasion of his late trivial illness, the subject of this communication was enabled to resume his ordinary avocations, and is now in the enjoyment of his usual health.]

ART. XVI.—*On the Radical Cure of Hydrocele of the Tunica Vaginalis Testis, by Iodine Injections; to which is appended a Parallel between Hydrocele and the Diseases with which it may be confounded.* By RICHARD G. H. BUTCHER, Member of Council; Fellow and Licentiate of the Royal College of Surgeons in Ireland; Late Examiner on Anatomy, Physiology, and Pathology thereto; Surgeon to Mercer's Hospital, &c.

THE radical cure of hydrocele of the tunica vaginalis testis is admitted by many practical surgeons to be most readily accomplished by iodine injections. The method was first prominently set before the profession by Velpeau in 1840^a, and from that time to the present his observations have been amply confirmed by the experience of others. Not only is the tincture of iodine capable of effecting a cure, when used in the first instance, but likewise its superiority has been proved in cases where every other method has failed. In England the attention of the profession was first awakened to the advantages accruing from this mode of practice, by a very excellent paper^b, from the pen of Mr. Martin, of London, who for many years resided in Calcutta, where the disease is acknowledged to be very prevalent. In this country I do not think the practice is as well known or as extensively employed as it ought to be. In very many instances I have adopted it myself, and uniformly with the happiest results. The surgeon may either employ the tincture of iodine pure or diluted with water, in the proportion of one part to three: the latter I prefer. The applicability of this mode of treatment to small hydroceles, probably, may be acceded to by all, while many will object to its use when the tumour is large; the object of this paper, however, is to show how safely and efficiently it may be employed, even when the sac contains pints of fluid, while the same small amount of the injection will answer in either case.

CASE I.—*Hydrocele of the Tunica Vaginalis Testis of the left side, together with acute inflammation of the Right Testicle, the result of Gonorrhœa.*—M. C., a car-driver, aged thirty-nine years, was admitted into Mercer's Hospital, March 18th, 1851. From his statement I discovered that a hydrocele of the left tunica vaginalis testis had existed for twenty years. He

^a Hydroceles de la Tunique Vaginale, Leçons Orales, vol. i. p. 262.

^b Lancet, 1841-42.

could not account for its occurrence, but steadily declared no violence had ever been inflicted on the part; that it came on at first imperceptibly, and increased without pain or uneasiness of any kind, very slow in its progress, and stationary as to size for many years; that for some months previously to his application at the hospital, its increase was more rapid, and then he was compelled to seek advice, owing to the inconvenience arising from its bulk. If his statement was true, not the least remarkable features in the case were the perfect transparency of the tumour, and the thinness and delicacy of the sac. At the time when the patient presented himself at the hospital, in addition to the chronic disease, he was labouring under acute gonorrhœa, which had manifested itself by profuse discharge three weeks before; during all this time he was taking active exercise, while very severe inflammation of the organ was produced, and suddenly, as it were, the discharge nearly ceased, and the right testicle became swollen and acutely painful. In this condition the man was admitted into hospital, and the requisite means taken to subdue the local affection; emesis was produced by tartar emetic, and its prostrating effects perpetuated by nauseating doses of the drug; pillows were placed between the thighs, and the inflamed part enveloped in warm stupe-clothes supported upon them. The hydrocele was remarkable in its size and form, being far larger than that usually met with, and somewhat contracted in the centre,—hour-glass shaped; its weight was also very considerable; the size of the tumour is best expressed by the quantity of fluid drawn off, which amounted to eighteen ounces. The palliative treatment being put in practice, the tension of the integument over the inflamed testicle on the right side was relieved, while at the same time the part was rendered more accessible to local management. On examining the inflamed gland, the epididymis was greatly involved, and contributed largely to the bulk of the swelling. True it is, Ricord states that, in the majority of cases, it alone is the part engaged, but my experience on this point does not harmonize with the opinion of this illustrious surgeon.

March 21st. Inflammation reduced; testicle smeared over with mercurial ointment, the part to be supported as before, and covered with warm stupe-clothes. Ordered five grains of blue pill at night, and an anodyne to procure rest.

23rd. Fresh exacerbation of pain in the testicle, from the gland being more swollen and its tissues more tense and binding. Blood was removed from the veins of the scrotum, and an

emetic given; these, in combination with soothing applications to the gland in the form of medicated stupes, afforded relief.

26th. Swelling and pain all gone; yet some hardness still remains in the epididymis. Discharge continues as profuse from the urethra as ever; ordered a drachm of powdered cubebs and ten grains of nitrate of potash every third hour; stupes to the part; omit mercurial.

29th. All pain in the testicle gone; one hardened nodule of lymph still at the junction of the epididymis and testicle.

31st. The powders have been regularly taken since last report, and the discharge is nearly stopped. There has been no return of inflammation in the testicle; it remains free from all pain, and of its normal size and appearance, save in the one point already referred to. The patient felt so relieved from pressing symptoms, that on this day he left the hospital, though cautioned to remain until the complete cure of the gonorrhœal discharge was effected. On the 12th of April following, this patient returned to hospital, with a fresh accession of inflammation in the right testicle; the disease assumed the most acute form; the gland was enlarged to the size of a lemon; the integuments were red and strained over it, all rugæ of the scrotum obliterated. The patient stated that the discharge was continuous from the time of his leaving hospital to the present period of his readmission; that for a few days before the testicle swelled, the evil had almost disappeared, and he considered himself on the eve of being cured: however, after suffering intense pain in the chord for some hours, the testicle too became effected, burning heat settled in it, and a deep, heavy pain, not nearly so acute as in the parts already referred to: yet with the dull sensation in the gland was associated the peculiar sense of nausea and faintness which so strikingly accompanies inflammation of this organ. Coexisting with this very inflamed condition of the testicle was profuse discharge from the urethra, the clap having returned with renewed virulence; the hydrocele on the left side was larger than when the palliative treatment was had recourse to, nearly a month before. The sufferings of the man were so great I at once opened the veins of the scrotum, and procured a large quantity of blood from the part; this was followed up by an uninterrupted succession of warm stupe-clothes, together with the support of the organ; a brisk emetic was afterwards administered, and a calomel purge given at night.

13th. Pain has again returned in the testicle, though the

bulk and tension of the organ are obviously lessened. Ordered tartar emetic in nauseating doses, low diet, warm stupes, and support to the part.

15th. Continued the depressing treatment up to this morning, with manifestly good effect, the pain having nearly all subsided, and the bulk of the tumour greatly diminished. To continue tartar emetic mixture, &c., &c.

18th. The testicle and epididymis now bear handling without much pain, but considerable hardness remains in the latter; therefore I considered it advisable to introduce mercury into the system, and cause the absorption of the lymph matting the tubes of the epididymis together, threatening an interruption to their functions. Two grains of calomel and a quarter of a grain of opium were administered every third hour, while the testicle was smeared with mercurial ointment, and then covered with lint and oiled silk.

24th. The treatment reported on the 18th has been uninterruptedly continued up to the present date, and with admirable effect. The mercury acted legitimately, the gums being slightly touched. The testicle has become diminished in bulk, the epididymis softened, and the hardened nodule of lymph nearly removed, and, not only so, but the discharge from the urethra is completely cured. To continue one pill at night, and the inunction of the gland as before.

28th. On this date the entire swelling of the gland has been removed, but the head of the epididymis still retains an abnormal hardness. Omit mercurial internally.

On the 30th of the month, the testicle being restored to its normal state, free from inflammation, I decided on performing the radical cure of the hydrocele of the left tunica vaginalis, by injection with iodine. The strength of the injection was the following: one part of the tincture to three of water. The fluid from the tumour was drawn off in the usual way by a trocar and canula; through the latter a drachm and a half of the above mixture was thrown into the tunica vaginalis by means of a glass syringe, and suffered to remain: on withdrawing the canula and syringe, a few drops of the fluid made their escape. In a few minutes after the injection was thrown in, the patient complained of pain in the part, accompanied by faintness. The pain extended down the thigh, and as high as the crest of the ileum, but never to the loins. The pain was very acute, and did not subside for five hours after, though a large opiate had been administered.

May 1st. Since the injection, as usually occurs, rapid effu-

sion has taken place into the sac of the tunica vaginalis; the scrotum is likewise œdematous; the pain and sickness, so intolerant on yesterday, have entirely subsided. The part to be supported by a suspensory bandage.

3d. Swelling and pain less.

12th. Tumour gradually diminishing.

14th. Fluid in the tunica vaginalis greatly absorbed, and the bulk of the swelling remarkably lessened; scrotum quite natural, and flaccid.

22nd. All fluid nearly absorbed.

27th. Not the least tumour now, all fluid being absorbed, and both testicles healthy; and on the 2nd of June the patient was dismissed, perfectly cured.

The chief complication in this case of hydrocele which required surgical interference, before the radical cure could be attempted, was, the acutely inflamed condition of the testicle on the opposite side. The means which I employ, and which I have found most decisive in arresting acute testitis, were employed here, namely, first, opening the veins of the scrotum. If this operation be carefully done, a very large quantity of blood can be drawn in a few minutes. To insure a sufficient supply, the surgeon should be careful not to make too much pressure on the chord, lest the arterial supply should be stopped; at the same time, it should be sufficient to check the returning flow, and render the veins prominent. Again, in opening each vein, the operator should be cautious not to transfix the walls of the vessel; if so, the fine cellular tissue behind is projected into the vein, and plugs the opening. In order that the blood should flow freely, the anterior wall of the vein must alone be cut. If a few vessels be opened, with these precautions, a sufficient supply of blood will be readily afforded: even though the scrotum be œdematous, as is sometimes met with in these cases, yet, one or two vessels will almost invariably be found towards the raphe, by the opening of which, the neighbouring veins, though concealed, can be effectually emptied. I far prefer this method of detracting blood from the part, to the application of leeches: 1st. Because it can be put in practice more quickly; and, when secrecy is required, without leaving any trace behind. 2ndly. Erysipelas is not at all likely to occur. I have never seen it as a result; while, after the application of leeches, it frequently takes place. 3rdly. By the method which I advocate, the patient is saved from the irritation of the leech-bites, which keeps up a constant flow of blood to the part, producing absolutely the effect which the leeches were intended to remove. After the

veins were opened, blood was encouraged to flow by the constant application of warm stupe-clothes; a full emetic was next administered; aperient medicines given; and the action of the heart controlled by nauseating doses of tartar emetic; the advantages of position were not lost sight of; and, finally, mercury was exhibited, to arrest the further exudation of lymph, and cause the absorption of that already thrown out, both in the epididymis and testicle itself. This is a practice which I invariably have recourse to, namely, where nodules of lymph, or undue hardness remain in the testicle or epididymis, after acute inflammatory action, I at once administer mercury, with the very best effect, and the restoration of the parts to their normal state. It is most important, I conceive, to aim at this result, that the matting together and occlusion of the seminiferous tubes may be prevented, and the functions of the gland preserved, and that no nidus be left for the generation of future disease.

The most remarkable features of interest in this case are:—1st. The radical cure of the hydrocele by the stimulant injection of iodine, without exciting fresh inflammation in the testicle so recently cured; 2ndly. The cure of the urethral discharge under the influence of the mercury; and 3rdly. The absorption of the effused lymph deposited in the tissues of the gland, by the administration of the mineral.

The next case presented a tumour considerably larger than that now detailed, and was treated in a similar manner.

CASE II.—J. R., aged 28, was admitted into Mercer's Hospital under my care, in the month of January, 1853, labouring under hydrocele of the tunica vaginalis testis. The patient stated that, three years before his admission, the swelling commenced, apparently without cause, as he was perfectly unconscious of having sustained any injury of the part; it gradually increased for two years and nine months, attaining such a size as to become troublesome from its bulk, interfering with his ordinary occupation, and permitting very imperfectly the act of coition. At this time the palliative treatment was had recourse to, and an amount of fluid drawn off, exceeding three pints and a half; in a few days after, the patient returned to the country.

In twelve weeks from the date of this operation, the patient came back to town, and, on the 16th of January, was received into hospital, the tumour being, if anything, larger than before. I admitted him with the determination to try the radical cure by iodine injection. The tumour, on examination, presented a large oval mass, implicating the ring. The skin over it was

smooth, tense, of the natural colour, and traversed by large veins. The two upper thirds of its extent were transparent, while the lower third was of a reddish hue, the result of œdema, consequent upon the exertions which the patient was compelled to make in walking. In feel it was elastic, fluctuating, uniformly tense, except at the back part, where it was hard in the site of the testicle. Its weight was less than that of a solid disease of the same dimensions. On handling it the sensations were not painful, except when pressure was made behind, corresponding to the position of the gland. The dragging on the chord in the erect posture constituted the summit of his sufferings. The penis lay embedded upon and lost in the upper part of the tumour, from the dragging of the integuments downwards.

January 17th. Having decided on trying the radical cure on this morning, I drew off the water, which was of a straw colour, and amounted in quantity to nearly four pints. The testicle being discovered perfectly healthy, I injected two drachms of the following mixture:—Tincture of iodine, three drachms; hydriodate of potash, one drachm; water, one ounce. The fluid was suffered to remain, and the canula withdrawn. In an hour and a half after the injection the iodine could be detected in the urine; it disappeared by degrees, and in forty hours after, no trace of it could be detected. After the injection I moved freely the fluid all round the surface of the sac, so as to bring it into contact with every part, and then ordered the patient to bed. Half an hour had scarcely elapsed when pain of a moderate degree commenced in the testicle, passing upwards along the chord. This continued for about two hours, but never produced sickness of the stomach, or any violent suffering.

3 P.M. Slight effusion into the tunica vaginalis, and also into the tissues of the scrotum; testicle tender to the touch.

January 18th. Has suffered no greater uneasiness than noticed on yesterday, yet the effusion into the tunica vaginalis and œdema of the scrotum are considerably increased. In addition to the tenderness and fulness of the testicle, the epididymis is likewise enlarged.

26th. All pain gone. Fluid absorbing but slowly, so drew it off again. Sac thickened, and testicle enlarged. No pain of any amount. Strapped the testicle, and administered five grains of blue pill night and morning.

February 6th. Since last report the testicle has been strapped on four or five occasions. The mouth now is slightly

touched by the mercury, and to the combination of both modes of treatment may be attributed the diminution of the swelling of the gland.

10th. Strapped the testicle to-day with a combination of ammoniacal and mercurial ointment, with soap cerate spread upon linen. There has been no tendency to the secretion of fluid into the tunica vaginalis.

15th. On this day the patient was dismissed from hospital, the parts being entirely restored to their normal state, and the hydrocele cured.

The only accessory called into requisition in the cure of this case, in addition to those used in the former, was the application of strapping the testicle. This is a mode of treatment which may be adopted even in acute cases of testitis, with the best results, after local depletion, in the way which I have pointed out. It is highly serviceable in cases that assume the chronic form, particularly when the patient is compelled, by the nature of his occupation, to move about. However, much of the benefit will depend upon the way in which the strapping is performed. In order that it should act beneficially, the pressure must be equal and considerable in amount. The testicle being drawn down, I first apply a piece of lint in a circular manner at the lower part of the chord; over this the first strap of soap plaster is wound—the object being to prevent the retraction of the testicle when pressure is applied to it. I interpose the lint, because I have frequently seen the upper edge of the plaster cut the integuments. No intervals should be left between the numerous straps afterwards put on, and if their pressure be exerted evenly, seldom, indeed, will the patient suffer any pain after their adjustment; and, I will further add, I have never seen their removal demanded, except when clumsily and imperfectly applied. For the purpose described, the soap cerate is spread upon linen, and cut into strips of about an inch wide; or a more stimulant application may be substituted, as above noticed.

The foregoing cases demanded a considerable time for their cure, owing to reasons assigned; but in the generality of cases treated by iodine injection, I have seldom seen it necessary to confine the patient to bed;—it is of all others the practice upon which most dependence can be relied for the radical cure of hydrocele.

To these remarks I shall append some Tables, showing the diseases with which hydrocele may be confounded, and the characteristic distinguishing features of each, so as to lead to greater accuracy in differential diagnosis.

DIFFERENCES BETWEEN HERNIA AND HYDROCELE.

HERNIA.	HYDROCELE.
Begins above.	Begins below.
Changeable in bulk.	Unchangeable.
Engages ring.	Ring free.
Feeling of weakness.	Feeling of weight.
Can often feel intestines, or omentum.	Can feel nothing.
Testicle at the bottom.	Testicle at back part.
Opaque: in child sometimes transparent.	Often transparent.
Base of tumour above.	Base of tumour below.
Flatulence, dyspepsia.	Bowels not deranged.

DIFFERENCES BETWEEN VARICOCELE AND HYDROCELE.

VARICOCELE.	HYDROCELE.
Soft, like earth-worms.	Tense, elastic.
Changeable, like hernia.	Unchangeable.
Ring dilated often.	Ring closed.
Testicle distinct.	Testicle indistinct.
Testicle wasted.	Enlarged, if distinguishable.
Tumour whole length of chord.	Tumour at bottom.
Tumour light.	Tumour heavy.

DIFFERENCES BETWEEN VENEREAL TESTICLE AND HYDROCELE.

VENEREAL TESTICLE.	HYDROCELE.
Both engaged generally.	One tunica vaginalis generally.
Tumour very heavy.	Tumour not so heavy.
Hard all over.	Hard only at back part.
Size moderate.	Often very large.
No fluctuation; sometimes small quantity of fluid.	Fluctuation.
Tumour slanting.	Tumour perpendicular.
Painful to handling.	Not painful.
Solid contents.	Fluid contents.
Eruption, or sore throat.	None such necessarily.

DIFFERENCES BETWEEN SCROFULOUS TESTICLE AND HYDROCELE.

SCROFULOUS TESTICLE.	HYDROCELE.
Round in form.	Oval in form.
Never very large.	Often very large.
Solid.	Fluctuating.
Heavy.	Light.
Lies at the bottom of the scrotum.	Grows upwards.
Inflames in spots.	Never so.
Suppurates, fungates.	Never suppurates.
Scrofula in other glands.	Not so.

DIFFERENCES BETWEEN FUNGUS HÆMATODES AND HYDROCELE.

FUNGUS HÆMATODES.

HYDROCELE.

Tumour irregularly hard and soft, hardness predominating in early stages.	Uniformly smooth.
Shape, globular generally.	Oval generally.
Rapid in growth.	Slow in formation.
Painful.	Free from pain.
Opaque.	Transparent.
Elastic.	Fluctuating.
Chord becomes hard and knobby.	Chord sound.
Pains up loins.	No such pains.
Health impaired.	Not so.
Fungates.	Never.

DIFFERENCES BETWEEN CANCER OF THE TESTICLE AND HYDROCELE.

CANCER OF THE TESTICLE.

HYDROCELE.

Hard, knobbed.	Soft, smooth.
Small.	Large.
Round.	Oval.
Painful on handling.	Not so.
No fluctuation.	Fluctuation.
Chord knobby.	Chord soft.
Adheres to scrotum.	Never.
Glands in groin enlarged.	Never engaged.
Shooting pains.	Never (in loins).
Fever peculiar.	No fever.
Fungates.	Never.
Death.	Never.

In hydro-sarcocele the testicle will be found hard, painful, irregular, large at the back part, with some fluctuation in front. Testicle distinguished in hydro-sarcocele, not so in hydrocele generally. Shooting pains on handling the former, not so in the latter. If obscure, the tumour may be tapped, and then the enlargement of the testis will be discovered, and the water small in proportion to the size of the tumour.

ART. XVII.—*Reports of Rare Cases observed in the Clinical Wards of the Whitworth and Hardwicke Hospitals.* By SAMUEL GORDON, M. B., F. R. C. S. I., M. R. I. A., Physician to the Hospitals.

(Continued from Vol. XV. p. 371.)

III.—*Empyema treated by extensive external incision; Recovery.*

ROBERT BRADY, aged 36, a house-painter, was admitted into the Whitworth Hospital, December 20, 1853. Eight days previously he had rigors followed by irregular perspirations, acute pain in the left mammary region increased by deep inspiration, dry cough, and loss of sleep; these symptoms still continue; the inferior third of the left side of the chest is quite dull on percussion posteriorly, and there is no respiratory murmur audible in this situation.

The friction sound is audible over almost the entire left side of the chest anteriorly, and extends considerably across the mediastinum. It is of a very peculiar character; examining it attentively at the right of the sternum, it is distinctly audible with the motions of the heart, the to-and-fro sounds being heard with each impulse of the heart, and continuing while respiration is interrupted. On the left side it is audible with the action of the lung, being heard twice distinctly with each act of respiration; the respiratory murmur is puerile throughout the right lung, and very feeble at the apex of the left. The heart is displaced to the right side of the sternum; the intercostal muscles on the left side protruded, and the left ala of the diaphragm depressed; the patient lay altogether on the right side; he had considerable fever, pulse 120. Tongue covered with yellowish fur; respiration very hurried; great anxiety of countenance. Blood was taken by cupping from the left side, and he was ordered calomel and opium in frequently repeated doses. He suffered greatly from local pain over the heart, which was greatly relieved by frequent leeching, and in addition to a fourth of a grain of opium, taken with calomel; every two hours he required a large opiate each night to procure some sleep.

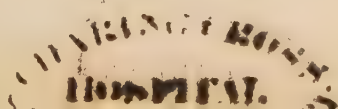
On the 24th, the calomel had affected the system, and from this time the pain completely left the region of the heart, and the friction sound gradually disappeared, the heart's sounds becoming daily more distinct. The mercurialization of the system, however, did not seem to have any effect in curing the disease of the pleura; the fluid gradually rose and compressed the lung still further; the extent of dulness increased; the *frotte-*

ment disappeared, and the respiration at the apex of the lung became bronchial. The type of the fever altered, and from inflammatory became hectic.

January 2, 1854, report as follows:—Complains greatly of debility; copious perspirations both by night and day; expectoration profuse, purulent, at least a quart in the day; the least exertion, even turning in the bed, brings on the cough, and the pus comes up in mouthfuls without much apparent effort; he lies altogether on the left side; pulse 132, very feeble. A loud muco-crepitating râle is audible near the spine of the scapula; slight bronchial respiration is audible still at the apex of the left lung, no respiration over the remainder of this lung, and perfect dulness on percussion. All the signs of displacement continue. He was now blistered repeatedly over the left side; he was ordered large doses of iodide of potassium, and a small quantity of wine.

On the 7th of January the dyspnœa was very urgent, the expectoration very profuse, he had coughed up a pint of purulent matter in five hours. On examining the side I found a puffy tumour about the size of a turkey's egg flattened, in the lateral region, corresponding to the ninth and tenth ribs; it had a deceptive feeling of fluctuation, and was very tender on pressure.

My colleague, Dr. M'Dowel, now saw the case with me, and finding that all therapeutic measures had failed, and that the dyspnœa was most urgent, we agreed on the propriety of making an incision into this tumour. I penetrated three inches in depth, but found no purulent matter. A great quantity of bloody serum exuded. I did not open the pleura because there did not appear to be any attempt at a pointing of the empyema at this situation; secondly, on account of the great depth of the pleura from the integument; and lastly, on account of the great irritability and impatience which the patient himself now expressed. I enlarged the wound, however, very considerably, and dressed it from the bottom, with the hope that the empyema would be thereby induced to point and discharge itself at this situation, treatment similar to that so well advised and frequently successful in cases of abscess of the liver. Beyond the usual amount of suppuration from the wound itself, no purulent matter ever came through this opening, yet I was most agreeably surprised to find that from this day the symptoms began gradually to amend, so quickly, however, after this interference, that it was obvious to all who witnessed the progress of the case, that the incision and the amelioration



of the symptoms must have stood in the relation of cause and effect.

The report of the 10th is:—Finds himself much better; can breathe with greater ease and freedom; expectoration much reduced; can turn or even sit up in the bed without coughing; perspirations continue; pulse 124; heart less to the right side than formerly; other physical signs unaltered.

The physical signs also did, however, soon begin to amend. The respiration at the apex of the lung became vesicular; the muco-crepitating rattle disappeared, and for several days the interesting phenomenon was well marked of the returning *frottement*, audible over a great portion of the left side of the chest, and the signs of excentric displacement disappeared.

He was discharged from hospital at his own request on the 28th of February, the cough and expectoration having ceased, and the hectic fever having subsided. He was very weak and emaciated; the left side of the chest was beginning to contract; its inferior part was still dull on percussion, and the respiratory murmur was but feeble.

On the 12th of March he had greatly recovered his strength; the perspirations had entirely subsided, and he only complained that he was still caught in the lower part of the left side by a deep inspiration.

March 31. He considers himself convalescent; has returned to his usual employment; complains only of slight fatigue at the end of the day. The left side of the chest is slightly contracted. There is still slight dulness and feeble respiration at the base of the left lung.

The nature of this case was evident from the commencement. It was clearly a case of pleuritis with effusion, combined with pericarditis. I do not think that this is a frequent combination, when attended, as in the present case, with high inflammatory fever. With the typhoid fevers, and cases of pyogenic diathesis, no combination can be more common. It has been stated that the occurrence of loud murmur in cases of displaced heart like the present is not unusual, and for a moment I thought such might be the case here, but the abnormal sound was at times peculiarly rough and rasping, not more audible towards or a little beyond the apex or base of the heart, but over its centre, and diminishing or almost lost towards these parts; its point of intensity so much to the right side showed, also, that the *frottement* was not owing to a pleuritis of the left border of the mediastinum, an affection sometimes not easy of diagnosis from pericarditis. The next

point of interest was the amount of displacement which had taken place very early in the disease. Before the left pleura was more than one-third full of fluid, the heart had gone considerably to the right of the sternum; the intercostal spaces had become convex outwards, and the left ala of the diaphragm had been depressed; each or all of these facts being sufficient to prove that these phenomena were not owing to mere mechanical pressure.

That the nature of the effusion was pus, and not serum, was, I think, sufficiently proved by the facts of—1st. The great amount of inflammation which existed, producing those displacements above mentioned, in so early a stage of the disease; 2ndly. The well-confirmed hectic fever which afterwards supervened; and lastly, by the great amount of *purulent matter* which was daily discharged from the bronchial tubes.

That this empyema was very large, I assumed not from the signs of excentric displacement alone, but from the great amount of pressure which both the symptoms and physical signs showed to be exercised on the left lung; the intensity of the dyspnœa, while there was no proof of disease in the right lung; the degree of hectic which existed, and the enormous amount of purulent expectoration. I would not go so far as Dr. Wood, who states, that large collections of pus in the pleural cavity are seldom, if ever, absorbed^a; but I think their absorption sufficiently rare to render such a case worthy of publication.

I need scarcely refer to the supposition that a communication might have been established between the lung and pleura, and the purulent matter so discharged through a bronchial tube. Such a circumstance I believe to be of exceeding rarity, more so even than the absorption of a large empyema; but such an occurrence could not have taken place without some signs of a pneumothorax, which never existed. But there were other physical signs of an exceedingly interesting nature present, and which I referred to above, namely, the bronchial respiration at the apex of the left lung, and the muco-crepitating râle near the spine of the scapula. These physical signs I had more than once known to be considered as indicative of phthisis, and the nature of the case completely mistaken by reason of their existence; but, recollecting the observations made by the late Professor Greene on this subject^b, from cases, all of which I had myself witnessed, I had no hesitation in stating

^a Practice of Medicine, vol. x. p. 44.

^b See vol. xvii. of the former Series of this Journal.

that they were owing to the compression of the lung, and the large collection of purulent matter which lay in the bronchial tubes; these tubes being in great number compressed into a very small space near the root of the lung, the sounds assimilated those which would be produced in an anfractuons cavity. And, lastly, to speak of the incision made, I confess I endeavoured at first to imagine that there was not any connexion between it and the subsequent process of absorption; but the one was so speedily followed by the other, that it was obvious to all who witnessed the case that, as I said before, the wound and the amelioration of the symptoms must have stood to each other in the relation of cause and effect; and such is also the opinion of Dr. M'Dowel, who saw the case with me on more than one occasion. Hodgkin, in recommending the operation for empyema to be performed by the cautery rather than by the knife, says a strong external irritation is one of the most powerful means of promoting the removal of the fluid by absorption. Whilst, then, we are preparing a way of escape for the effusion, we are at the same time giving to nature the best chance of removing it herself, and we may, perhaps, fortunately find that before an opening is effected, she has so well performed her part that the plan may be changed. Such I suppose to have been the mode of action in the present case, in which a very extensive and deep incision proved to be a strong external irritation, exciting the absorbents to more powerful action.

IV.—*Pleuritis with Effusion: Absorption: Occurrence of Pneumothorax during process of Contraction of Side.*

On the 7th of May, 1850, I saw a young woman of about 32 years of age. She had been for some time under treatment for a supposed affection of the stomach, but her case was clearly one of pleuritis with effusion of the left side, and it was of about three months' duration. All the phenomena of excentric displacement were present; also there was dulness over the entire left side of the chest, and over the anterior mediastinum, and no respiratory murmur was audible except at the root of the lung; she had a slight sense of weight and oppression, but no pain in the chest; short teasing cough, with but little expectoration; no symptom of hectic, except progressive emaciation. Under treatment the effusion was in a great measure absorbed; respiration returned to the upper portion of the lung, and œgophony became audible; the signs of excentric displacement disappeared altogether; and towards the end of August she was able to resume her ordinary occupations. I have been in the habit of

seeing this patient at intervals ever since; she was seldom more than three months without applying for advice, and on each occasion on account of pains and uneasy sensations in the left shoulder and lower part of that side; the least exposure to cold or any unusual fatigue always induced them, yet there was never any reason to suspect a re-accumulation of the fluid; on the contrary, there were each time I saw her evidences of further absorption; the symptoms were always alleviated by opiate applications to the side, and iodide of potassium internally.

On the 12th of October, 1853, her condition was as follows: the right lung had become enlarged, the anterior mediastinum being displaced to the left side; the respiratory murmur was puerile. The circumference of the left side was two inches less than that of the right; there was vesicular respiration audible over the entire left side, but it was very feeble, being most distinct posteriorly; the sound elicited by percussion was much less resonant than on the right side; the apex of the heart was in its natural position. She had a slight attack of bronchitis, which subsided in a few days, and I did not see her again until the 9th of January, 1854. She had then a slight return of the oppression of breathing, which she now referred to the *right* side. I was surprised to find that there was tympanitic resonance on percussion over the left side; there was also well-marked amphoric breathing, and a loud metallic ringing sound was produced by coughing or speaking loudly; these latter phenomena did not exist at the inferior part of the chest; there was no decided metallic tinkling, and succussion did not produce any splashing sound. The mediastinum was again displaced to the right side, but there were none of the other phenomena of excentric displacement, and measuring from the ensiform cartilage to the spinous process of the opposite vertebra, the chest still continued two inches smaller than the right. The general symptoms were very little altered from the time I had last seen her. She had no accession of fever, no increase of cough, no pain in the side, only a sense of oppression of breathing, and that very slight. She could lie equally well in any posture, but preferred lying on her back. On questioning her closely to find if I could refer the occurrence of the pneumothorax to a certain moment, she told me that on the night of the 6th she awoke with a pain in her left shoulder, and she fancied that from this time the breathing was a little oppressed, but she thought the pain in the shoulder was rheumatism, and it disappeared the next day under the use of friction. At this time Dr. Corrigan saw the patient, and agreed that it was clearly a case of pneumothorax, but without any proof of the presence of fluid in the pleura.

The question only remained then as to the cause of the pneumothorax.

The arguments against supposing it to have been the consequence of phthisis appeared to be quite conclusive. There was never at any time any reason to suspect the existence of phthisis, even before the occurrence of the pleuritis; and we know that the contraction of the lung after pleurisy tends greatly to the cure of the tubercular deposit rather than to its development *in the lung*; the great expansion of the opposite lung taking on a compensatory action is also an argument against the development of phthisis. Pneumothorax is frequently described as being caused by a pleuritic effusion, which is said to excite inflammation and ulceration in the pleura, and by the same processes to open a passage through the pulmonary parenchyma into the bronchial tubes, and so to be discharged. Such a mode of recovery I believe to be of exceeding rarity; almost all the effusions considered to have been so evacuated have, doubtless, escaped from the system by a vicarious secretion from the bronchial mucous membrane or a process of endosmosis, but that an occurrence like the above does occasionally take place is sufficiently proved by the case recorded by Archer in the Transactions of the College of Physicians, and others sufficiently authentic: that such was not the case in the present instance was obvious from the facts:—1st. That there was latterly no evidence from the general symptoms of any fluid remaining in the pleura, and as to the physical signs *œgophony* had long since ceased, and the test of position, which was at first very satisfactory, had latterly no effect in altering the degree or situation of the dulness, or feebleness of respiration. But, 2ndly, if the pleura had been perforated by such process to allow of the evacuation of fluid, or if fluid had been in the pleura at the time of perforation, this occurrence would have been followed by cough and discharge of fluid from the bronchial tubes, which has never yet occurred. And, 3rdly, it is absolutely proved that there has not been any fluid in the pleura since the occurrence of the pneumothorax by the complete absence of metallic tinkling or any splashing sound on succussion. I consider that the entire of the fluid pleuritic effusion which once existed in this case was absorbed; that the lung, so far from expanding in order to fill the space so left, was itself undergoing a process of compression from the amount of false membrane which had been poured out on it, and which, now organized, was, as is usual in such cases, contracting on itself, and leaving vacant a further portion of the pleura; the surrounding organs being unable to close in any further than they have

done, the vacuum has been filled by air, not secreted by the pleura (I doubt much the existence of any such case except in gangrene), but drawn by suction from a part of the lung least covered by adventitious membrane. Wood, in his "Practice of Medicine," endeavours to account for phenomena similar to the present in the following way. He says^a, "in some rare cases of partial pleurisy in which the liquid effusion is limited by adhesions, it is said that the place of the liquid absorbed is sometimes supplied by air, the lungs not being sufficiently expansible, nor the ribs sufficiently flexible, to fill up the vacuity. It is probable that the air in such cases is given out by the liquid, which always contains more or less of it under the ordinary atmospheric pressure, and yields it when that pressure is removed."

This form of pneumothorax is also alluded to by Williams^b: "After a pleuritic effusion has long compressed the lung, and the compression has been perpetuated by a rigid false membrane which has been formed over it, the absorption of the liquid leaves a void which the collapse or contraction of the walls of the chest is in some few cases insufficient to obliterate, and this void is sometimes filled with air secreted by the membrane. I have seen two instances of a partial pneumothorax apparently produced in this way. They each occupied about half of the pleural sac,—one the upper, the other the lower half,—and the lung in both cases was strongly bound down by fibro-cartilaginous membrane, and condensed to the part contiguous to the empty space. There was also some contraction of the chest in both cases."

The case at present under consideration is very similar to those mentioned by Williams—in the pneumothorax being partial, and the side being contracted,—but the morbid anatomy of the disease clearly does not consist in a secretion of air, as supposed by Wood and Williams. If this were the case, we would not have the sign of amphoric breathing, which is most distinctly marked, and is heard most loudly in the act of expiration. Dr. Stokes has described^c what he terms a cribriform state of the pleura, and says that he has frequently observed it in connexion with chronic pleurisy. He does not say, however, whether it was attended during life with any or what physical phenomena.

So long ago, however, as February, 1838, I recollect and have notes of his demonstrating this morbid condition of the pleura in a case which he then termed one of chronic

^a Vol. ii. p. 54.

^b Fourth Edition, p. 125.

^c Diseases of the Chest, p. 539.

pneumothorax, and this condition, or one very similar to it, I believe to be the true morbid anatomy of the disease which is usually termed pneumothorax by secretion. Extravasation of air into the pleura, in a case similar to the above, is analogous to what has occurred in the same membrane placed under different circumstances. After the operation for empyema, the entire of the fluid being withdrawn and air most carefully excluded, the lung, from previous compression, has been **unable** to fill the cavity, and the patient has suddenly died from the quantity of blood drawn by suction from the **surface** of the lung.

It remains to say a few words as to the treatment and progress of the present case. Although the affection supervened almost insensibly, without pain or stitch in the side, yet for some days she was strictly confined to bed and to low diet, and small doses of opium were frequently administered. When it was discovered, however, that no symptom of inflammation of the pleura set in, she was allowed to leave her bed, the opium was omitted, and she was ordered a small quantity of cod-liver oil at night, and milk diet.

The slight dyspnœa which she complained of has now declined. She is taking tartrate of iron in wine, and has found the greatest possible relief from the left side of the chest being tightly bandaged. She takes animal food daily, and is permitted to go about the house, but has not yet been allowed to go out.

There is still (April 4) tympanitic resonance on percussion, amphoric breathing, and metallic vibration of voice and cough over the upper half of the left side of the chest, but no indication of the existence of any fluid in the pleura, and the left side of the chest is still contracting. There is some vesicular respiration audible near the spine and inferior portion of the chest. Her appetite and general health are daily improving.

The chief peculiarities of this case seem to be—1st. Its supervention in a case recovering from pleuritic effusion, in which there had never been any sign of phthisical development. 2nd. Its supervention without any pain or sudden stitch in the side. 3rd. The complete absence of all symptoms and physical signs of inflammation of the pleura as a consequence of its supervention. 4th. Its partial character: the lung was never completely collapsed; there was always some vesicular respiration near the spine, and at the base of the lung. 5th. The absence of any sign of the coexistence of fluid in the pleura. 6th. The very slight inconvenience which the patient seems to suffer from its continuance; and lastly, the mode of

treatment which appears to give most relief being tonics and properly arranged pressure on the affected side.

V.—*Cirrhosis of the Liver: its different Causes: its occurrence in very young subjects.*

Richard Moffett, aged 15, was admitted into the Whitworth Hospital in June, 1853, for purpura hæmorrhagica; he was a thin, delicate-looking boy. His mother stated that he was a healthy child until about fifteen months old, when he was attacked with hydrocephalus, but this soon disappeared, and he enjoyed good health until he was five years of age. He was then seized with liver complaint, the sclerotic coat of the eyes became yellow, and the whole body was jaundiced; the urine was high-coloured, and the stools white and doughy. He remained in this state for twelve months, and was then sent to the country for seven months. He came home recovered, and continued in good health (with the exception of a chronic diarrhœa and epistaxis, to which he was always subject), and was apprenticed to a trade about two years ago. Twelve months since he complained of a general ailing, suffering from sickness of stomach, loss of appetite, and increased diarrhœa, with severe pain attending it. He seemed to recover, and resumed his occupation, continuing at it until a few weeks before admission into hospital; in addition to an eruption of purpuric spots over the entire body, he had then epistaxis and frequent bleeding from the gums. The spots were of various sizes, from the diameter of a pin's head to that of a large pea, and there were also several small greenish patches over the body, like contusions. The skin was peculiarly dry and harsh; he had no anasarca, ascites, nor local œdema; he remained in hospital five weeks, and was discharged convalescent; the purpura had entirely disappeared; he was treated principally with oil of turpentine, vegetable diet, and the warm bath. The epistaxis recurred from time to time, and he continued to present himself as an extern patient for the relief of this symptom, but during all this time he had neither jaundice, dropsy, pain in the shoulder or side, nor any other symptom from which disease of the liver could with certainty be inferred. On the 25th of November he came down stairs staggering, and complaining of lightness in the head. He also vomited a transparent liquid, followed by a little blood; shortly after, he seemed to recover, and ate a hearty dinner. On the 27th, he vomited a good deal of a reddish, brown-coloured liquid, like water mixed with coffee. During the day he recovered a little, and was able to walk about, and his appetite was tolerably good. On the 28th, at the same

hour, he again vomited a similar fluid, mixed with undigested food. He had a return of vomiting during the day, and a motion from the bowels; the fæces were of a very dark colour; he complained of great weakness. On the following day he applied for admission into hospital, complaining of nausea and pain in the abdomen; he vomited blood three times that day, and was so weak that having got out of bed he was unable to return to it; he expired at four o'clock the next morning.

Post-mortem Examination.—The body was well formed and rounded. A few spots of purpura were seen on the legs. The pericardium and heart were healthy, except a slight thickening of the aortic valves, and a few spots of purpura on both auricles. The lungs were also in a tolerably healthy state. In the apex of the left a few tubercles were found. There were a few spots of extravasated blood on the surface of both lungs. There was no effusion into the peritoneal cavity, and the omentum was covered with fat. The liver presented the appearances of cirrhosis in a very advanced stage. It was very much reduced in size, but much heavier than natural. Its surface was entirely nodulated. Its shape was altered, having now become like an irregular ball. Along the originally sharp edge the substance of the liver had retracted, leaving the thickened peritoneum projecting beyond it. Its colour was of a light amber, and its consistence exceedingly firm and tough. A section of it, under a magnifier, showed most remarkable hypertrophy of the connective tissue, and complete isolation of the acini of the liver. The gall-bladder was small, thickened, and pale. The spleen was slightly enlarged. The stomach was distended, and filled with coffee-coloured fluid, similar to what was vomited during life. The mucous membrane was slightly congested. The duodenum contained coagulated blood;—the jejunum was free from it; but from its termination to the end of the rectum, the intestine contained a great deal of it. There was no appearance of inflammation or ulceration of the mucous membrane. There was neither anasarca nor effusion into any of the serous cavities.

There are several points of interest in the above case. 1st. The complete absence of dropsy or any other leading symptom of the disease of the liver, which was so far advanced. 2nd. The youthful age of the patient. Baron and Gherard have, however, recorded examples of it in much younger subjects, and in December last there was under my care, in the Whitworth Hospital, a young lad, twelve years of age, with the following symptoms, which I consider pathognomonic of the disease:—Emaciation, with a light yellow earthy “jaune

terrense" hue of the skin; very great enlargement of the spleen; retraction of the liver, and slight ascites, with epigastric tenderness, and enlargement of the superficial veins of the abdomen. 3rd. The mode of the patient's death, by a sudden attack of hematemesis. Dr. Law (who asserts, moreover, that cirrhosis is the only disease of the liver in which hematemesis occurs), in the year 1829 drew attention to hematemesis as a symptom of cirrhosis of the liver, and as such it has been frequently observed; but it is rarely the cause of death. A remarkable case of this kind occurred some years ago in the Whitworth Hospital, under my observation. Thomas Enright was admitted, under the care of the late Dr. Greene, for cirrhosis of the liver; he was in hospital for some time, and was supposed to be much improved, when he died suddenly in the middle of the night. I examined the body on the following day: the liver presented the usual appearance and anatomical character of cirrhosis; the spleen was very much enlarged; the stomach was greatly distended, with black, coagulated blood, which had been poured out from the capillary vessels; there was no wound of any vessel, or other abrasion in the stomach. The specimen is still preserved in the Museum of the Richmond Hospital.

4thly. With regard to the cause of the disease, it is manifest that in the present case it was not produced by the ordinary assigned cause, the constant use of ardent spirits. This, however, is doubtless the most common cause; and here, as Dr. Budd says, the inflammation of the areolar tissue is probably owing to the diffusion of alcohol through it from the portal veins.

I have been able to trace cirrhosis of the liver to three other distinct causes:—

1st. Following a local peritonitis. In this case the hypertrophy or degeneration of the fibrous tissue of the capsule of Glisson is caused by an extension of the inflammation from the serous membrane in the same way as we observe the analogous disease to spread through the lung as a consequence of inflammation of the pleura.

2ndly. As a consequence of duodenitis. Such cases are at first attended with all the symptoms of acute hepatitis, from the rapid spread of inflammation along the ducts, but if these be neglected or improperly treated, they are followed by a low or chronic form of inflammation, which consists, in fact, in an extension of the disease to the fibro-cellular matrix, producing subsequently the usual evidences of cirrhosis.

And lastly, I have seen cirrhosis occur as a consequence of

enteritis of the lower part of the intestinal canal, gradually extending upwards. The progress of the disease in these cases differs from that in duodenitis in being always much slower, and not marked by any of the acute symptoms by which the former is indicated.

These divisions of the causes of cirrhosis account fully for the differences observed in various examinations in the several component parts of the liver and its adjuncts. In the present case it is probable that the disease commenced as inflammation of the duodenum, and that a neglected hepatitis, the consequence of it, terminated in cirrhosis of the liver.

ART. XVIII.—*Brief Remarks on some Points connected with Pneumonia*. By HENRY KENNEDY, A. B., M. R. I. A., Physician Extraordinary to Sir Patrick Dun's Hospital^a.

SEVERAL years have now elapsed since I took notes of a number of cases of pneumonia, which, occurring, as they did, within a very brief period of each other, might almost be called an epidemic of the disease. At any rate the cases presented some features of interest, which I have often had occasion to verify since; and which I purpose making the basis of a few remarks. I should say that the cases occurred all in the spring of the year, at a time when the sun was unusually hot, and the wind in the shade, piercingly cold. I would also wish it to be understood that the remarks are in truth "*disjecta membra*;" and only so far related, as that they all occurred in the one disease. The number of cases of which I took notes at the time amounted to 17; and it is worthy of remark that of these the disease prevailed in the left lung in 12; in 2 it was double, and in the remaining 3 it was confined exclusively to the right side. What determined the disease to show itself in this particular way, it would be hard to determine: for it is known to be directly contrary to the statistics of pneumonia brought out on an infinitely more extended scale, the right lung being the one most generally affected; nor in the particular instance given was the disease of the typhoid type. True, the numbers I have alluded to are very small, and yet I cannot but think the fact stated is a remarkable one, and worthy of record. I have seen nothing like it since.

A second point which the cases alluded to forced on my attention was, the time which elapsed previously to the develop-

^a Read before the Surgical Society of Ireland.

ment of the physical signs. This is a point which has been very commonly observed in reference to all kinds of fevers, where two or more days will elapse before we can pronounce as to the nature of the attack. But it has not been so generally observed upon in the acute phlegmasiæ, particularly those of the chest; and yet it is very common. I have now had occasion to observe it in a large number of instances. That is, we will have all the general signs of the disease, as, for instance, pneumonia, pleuritis, pericarditis, and even bronchitis, and yet we will not have the physical signs developed, on which alone we can pronounce with certainty. And thus forty-eight hours and even more will pass over, and we will be kept in doubt all this time. This point has been especially noticed in reference to pericarditis, and particularly by Dr. Mayne in his able Essay on that disease. But it also obtains in the other affections spoken of; pneumonia affording very marked examples of it. Some time back I saw, with my friend, Dr. Denham, a case of this sort, where three entire days elapsed before we could pronounce a case to be one of pneumonia. In this instance, besides sharp feverish symptoms ushering in the attack, there was also marked vomiting at the commencement; a symptom little likely to lead to a correct diagnosis. I may mention, too, that exactly the same occurrence often happens in pneumothorax, where, though we have the strongest presumptive proofs of the accident having taken place, yet thirty-six or forty-eight hours may elapse before the physical signs are evident. It may be observed in passing, that while examining the lung, in which physical signs of pneumonia were afterwards developed, the respiration was almost invariably weaker than on the opposite side; and I cannot but think, though high authority has stated it, that a puerile respiration, at this stage, must be a very exceptional case indeed.

From what has preceded, then, it will be understood why it has appeared to me necessary to direct more attention to what may be called the initiatory stage of the acute phlegmasiæ of the chest, and more particularly of pneumonia, than it has hitherto received. The diagnosis of disease is too important a point to neglect any facts which tend to throw light on the difficulties which surround it, and what has been stated bears on it directly.

The seat of pain in pneumonia, when it exists, for it does not always do so, very generally corresponds to the seat of the disease. I have now, however, seen a number of cases where this correspondence did not take place; and I believe the point worthy of notice. I am not now speaking of the period before

the disease has declared itself; where, as is well known, flying pains in different parts are often complained of, but of a period when the disease is fully formed. Thus I have repeatedly seen the pain complained of in the usual site, low down on either side, or under the breast, and yet the disease itself has been at the top of the lung; and in some instances there has been evidence of healthy lung intervening between the diseased portion at the top and the seat of the pain. Again, I have met, though rarely, cases where the pain was referred to one side, whilst the disease was situated at the opposite. One of the most remarkable of such cases occurred very lately in Cork-street hospital, under the care of my friend, Dr. G. A. Kennedy. In this instance a man named Grady, eighteen years of age, was admitted with very sharp fever, and complaining of severe pain in the left side, in the usual site. He lay in bed inclined to the right side, but not lying completely on it. On examination I found the left lung affected with puerile respiration from top to bottom, and with this a very clear sound on percussion. In fact there was no sign whatever of pneumonia affecting this lung, *though he had been some days ill* on admission. On the right side, however, I must say I was somewhat surprised to find the lung solid, as it is erroneously said, from top to bottom; in fact affording a marked example of tubular respiration^a. Although I examined this man several times subsequently, I never detected any change in the physical signs on the left side. The cause of the pain, in both the class of cases, is probably due to the presence of some amount of pleuritis, though physical signs may not always be present to show this. I have sometimes thought, however, that it might be due to some spasm of the intercostals, being the affection known as pleurodynia.

Speaking of the seat of pneumonia, I would allude to a case which I saw several years since, and appears worthy of being put on record.

A girl of 16, five days ill, was admitted into the hospital labouring under a very serious illness. Her pulse was upwards of 140; her breathing very hurried, and she was raving. On examination, dulness was found in the right mammary region, and this reached the clavicles. Posteriorly, however, corresponding to the dull region in front, the sound on percussion was remarkably clear. To the stethoscope, tubular breathing was heard in front, while posteriorly it was natural, but pos-

^a On another occasion I have adverted to the fact that the lung does not become absolutely solid in any form of pneumonia, though from the accounts in books this might be inferred. Some of the bronchial tubes always remain open, and afford some respiration during life, and may be always seen after death.

sibly a shade stronger than usual. There was not a trace of crepitus anywhere. The girl died on the night of the day she was admitted; and on examination, a fine specimen of pneumonia, in the second stage, was found; but it was confined exclusively to the anterior surface of the middle and upper lobes of the lung, the posterior being quite healthy. The lower lobe was not engaged at all. I have given this case as affording an unusual modification of the seat of pneumonic disease, and as such, worthy of note. I have also seen the base of one, and the top of the opposite lung, affected at the same time.

The expectoration of pneumonia has, from the time of Laennec, attracted much notice; nor am I going to speak of it now at any length. I would just observe, that it often affords a confirmation of the point with which I began these remarks—viz., that the physical signs may not exist for one, two, or three days, after the attack has commenced. Just in the same way there may be no expectoration for many hours after the disease has shown itself, even though at the time there is cough. Cases, too, occur where, though there is expectoration in the first instance, still it does not assume the character of rusty sputa till some time later in the disease. The total absence of sputa during the whole disease has also come under my notice, even in adults; and this has been observed by others. There is, however, one symptom connected with the expectoration which has several times come before me, and which calls for more notice than, I believe, it has hitherto received. I mean hemoptysis, as quite separate from rusty sputa. This may occur to a very considerable extent. Amongst the 17 cases to which I have so often alluded, it took place in no less than 3; and in one of these a large quantity of blood was lost. Since that time, I have often seen it in solitary cases. It is one of the few instances in which hemoptysis may occur in a man, and yet be of comparatively little moment. There will be occasion to allude to this occurrence again.

On the subject of crisis occurring in the progress of pneumonia, I need scarcely speak again; for on a late occasion, special attention was directed to it. I shall only add, that since then, two examples came under my notice in the Cork-street hospital. They occurred in the same week, began very nearly at the same hour in the morning; and one, especially, was really as marked an example of the occurrence as I ever saw in essential fever. In both instances, the crisis was by perspiration; and in one there was, in addition, a copious deposit in

the urine. As a whole, I cannot but look upon the fact as an important one in the history of the disease.

Dr. Addison, of London, has drawn special attention to the presence of what is known as "*calor mordax*," as diagnostic of pneumonia. It has often come under my notice; and in the 17 cases already spoken of, it existed in the great majority, and in a marked degree. On the other hand, I have seen a much greater number of cases where it was not present; many of these, too, where, with livid extremities, the temperature of the surface was actually below the healthy standard, and afforded no biting sensation to the hand. Neither is it confined, as all must be aware, to pneumonia. I have met with it often in scarlatina, in erysipelas, in ordinary fevers, and, more especially, the febrile affections of childhood. For these reasons, then, I would certainly not give it the prominent place Dr. Addison has; for, from reading his essay, one would really be led to think it was the *sine qua non* of pneumonia.

The complications of pneumonia are amongst the most important parts of the subject. Yet it is my intention here to notice one only, to which, some time back, I briefly directed attention. Each year since, however, is adding to the importance of the complication, and showing me it is one which calls for more special attention than, as far as I am aware, it has hitherto received,—I allude to pneumonia complicated with affection of the brain. Hippocrates notices the fact of raving in the progress of the disease; and he makes it a fatal symptom. This is going farther than is quite correct, at least, in this city; for I have seen a good many cases now where patients recovered after having exhibited it, and who raved even while awake. It is not the mere existence of raving, however, to which I would now direct attention, but the fact that the symptom is often due, in such cases, to actual disease of the brain or its membranes. It is now several years since I had occasion to examine a case where, during life, raving existed. Being curious to ascertain the state of the brain, I opened the head, when, to my surprise, I found lymph effused in considerable quantity—in fact, a genuine arachnitis. This I have met different times since; and more especially, when pneumonia affects the upper lobe of the lung. Hence one reason, at least, why this latter affection is so much more fatal than ordinary pneumonia, as probably every one is aware. Hence, also, the importance of ascertaining whether the raving be merely sympathetic, or due to actual disease. Hence, too, the question of treatment, and especially in the latter case. In

fact, the existence of disease in the brain alters, in every point, our views of the subject. But this may go still farther; and actual paralysis may happen in the class of cases I am now speaking of. For the notes of the following case I am indebted to my friend, Dr. Albert Walshe:—

A young man, aged twenty, rather delicate, was attacked with feverish symptoms, which were treated as fever for eight days. Dr. Walshe then first saw the patient, and detected pneumonia affecting chiefly the upper lobe of the left lung. The fever ran very high, and there was “*calor mordax*.” The patient also complained of headach, and had raved during the night. By means of local treatment, and calomel and opium, the symptoms subsided a good deal, and the patient expressed much relief as regarded his head. The pulse, too, which had been 130, fell to 96. Two days later, however, he began to complain again of his head, and a day later still the right side suddenly became paralytic. The tongue was protruded to one side, and he was unable to speak. From this state, though active remedies were used to the head, he never rallied. The physical signs in the chest improved a good deal before death, which took place within forty-eight hours of the occurrence of the paralysis. There was no post-mortem examination. There can, however, be little doubt, from what has gone before, that serious mischief existed in the brain, and of the acute kind.

In connexion with this particular point, that is, the co-existence of affection of the brain with disease in the chest or abdomen, I may mention that something very analogous occurs, and not unfrequently, in chronic disease. Thus, in some cases of *tabes mesenterica*, after it had existed for months, I have seen the signs of the disease lull, as it were, and then be succeeded by disease of the brain, of the nature of *hydrocephalus*. And again, in *phthisis*, but this is more rare, the very same thing has come under my notice; and it was really curious to observe, in both these complications, how very latent the original disease became. The occasional connexion of *phthisis* with disease of the brain has been specially written on, in a paper which I recollect reading many years since. On the whole, this complication of diseases in the chest and brain seems to me one of considerable importance.

On the treatment of pneumonia I have little to say. I would, however, venture to call in question the tendency, if it be nothing more, which is beginning to prevail just now—I mean the idea that bloodletting of any kind is injurious, and which has been advocated by Skey, Todd, Bennett, and others. I am satisfied that these opinions may be carried too far; and

that in some instances general and, in by far the majority of instances, local depletion may be used with the most marked advantage. Cold extremities and a weak pulse at the wrist are not to deter from active treatment, provided there be evidence of acute pneumonia in its first stage, and of short duration. The result of depletion, of whatever form, in such cases, is too marked to admit of doubt. The relief expressed by the patient; the development of the pulse; the very increase of the animal heat itself, all show that these are due to the depletion. Two facts also, already spoken of, appear to me to bear strongly on this question. The first is, that I have seen distinct crisis occur after a marked antiphlogistic treatment. I am not sure that any single argument could be advanced which would confirm the value of active treatment more distinctly than this; for, had the treatment been injudicious, it would surely have interfered with such a process as crisis. The truth is, that in the proper cases for antiphlogistic treatment, the result is to lower the excitement of the system, which was too great to allow a process like crisis to go on. It does not appear possible to explain the fact on any other supposition.

The second point which I think confirmatory of the propriety of a depletory treatment in the proper cases has been already alluded to—I mean the existence of hemoptysis. If nature herself relieves the lung by a direct outpouring of blood, it appears to me, the practitioner will rarely err, who with ordinary judgment has recourse to the same plan.

In concluding these desultory remarks, I shall venture to throw into a series of propositions the points which have appeared to me worthy of being brought under notice.

1. That the general signs of pneumonia often precede the physical, by a period of from one to three days.

2. That pain may exist low in the side, though the disease be at the top of the lung.

3. That pain may exist in the side opposite to the disease, and would appear to be, at times, due to spasm of the intercostal muscles, rather than pleuritis.

4. That pneumonia may affect the anterior surface of the lung, leaving the posterior free.

5. That hemoptysis may occur in pneumonia quite independent of the existence of tubercles, and quite distinct from pneumonic sputa.

6. That distinct crisis is by no means uncommon in pneumonia.

7. That “calor mordax” is not at all a necessary symptom of the disease.

8. That the raving which so frequently attends pneumonia is often due to the existence of acute disease in the brain.

9. That this disease of the brain may even lead to paralysis.

10. That the antiphlogistic treatment, including bloodletting, and modified according to the demands of each case, is the best treatment which can be adopted in the early stages of the acute and sthenic disease.

ART. XIX.—*Contributions to Midwifery*, No. VIII.—*Chloroform in Parturition*^a. By THOMAS EDWARD BEATTY, M.D., M.R.I.A., Professor of Midwifery to the Royal College of Surgeons in Ireland; Physician to the City of Dublin Hospital; Vice-President, Dublin Obstetrical Society; Honorary Member, Obstetrical Society of Edinburgh, &c.

IT may be in the recollection of some of the members of the Obstetrical Society, that in the last paper on the use of chloroform in midwifery, published by me, I stated my belief that there was no case of ordinary labour in which it would be found necessary to produce complete sopor, but that the mode and extent of administration set forth therein would be quite sufficient in all cases to procure the relief sought for by the use of the drug. A very short time elapsed, after the publication of that essay, when I met with the following case, which served to show me that the opinion therein advanced was not strictly tenable, but that cases may and do arise—rarely, I believe—in which it will be necessary to deviate from the rule laid down, and instead of the small and often repeated doses by inhalation of chloroform, so as to secure freedom from pain without deprivation of consciousness, a full and free use of the drug will be required to overcome the highly exalted state of the nervous system, and procure repose.

October 2nd, 1852. Mrs. —, aged 23 years, a thin, spare, delicate, highly nervous and excitable person, was taken in labour of her first child at 11 o'clock, P.M. The pains increased gradually for four hours, when the os uteri was nearly dilated, and the head of the child was well advanced through the cavity of the pelvis. She was very anxious to have the chloroform, and accordingly the inhaler was used in the manner so often described by me. She continued the use of the drug

^a Read before the Dublin Obstetrical Society.

for six hours without intermission, and without any signal benefit or relief. She seemed to suffer more pain than falls to the lot of the great majority of patients, and she was less influenced by the inhalation than I had ever witnessed. She dashed herself about with uncontrollable violence, and notwithstanding she had inhaled fully four ounces of chloroform in six hours, given through the inhaler, she resisted its anæsthetic influence. At this juncture I saw that my usual mode of administration would not do, and that something more decisive must be resorted to. The pains were violent; the head had just come to press on the perineum; the prospect of delivery in a short time was but faint; I therefore determined to get her at once completely under the full action of chloroform. With this view, I placed a piece of sponge in the bottom of a tumbler, and, having moistened it with the drug, I applied the mouth of the glass over the mouth and nose. Her violence was soon subdued, and she fell into sleep, in which condition I kept her for four hours, during which the pains continued with equal violence as before. At the end of that time, finding the head had not made advance through the pelvis, I had recourse to the forceps, and delivered a very large living boy. I was struck with the rapidity with which this lady recovered consciousness after the chloroform was withdrawn from her. In a quarter of an hour after the child was born, she was the only person in the house who could remember the name of the woman who had been engaged to nurse the child, and where she lived. This showed that her sensorium was not materially or permanently affected by the enormous amount (6 oz.) of chloroform she had employed during ten hours of her labour.

I have since met with two or three patients whose tolerance of chloroform was nearly equal to that just described, but none to come up to it.

The following case exhibits in a striking manner the value of chloroform in one of the most perilous conditions attendant on parturition, namely, *puerperal convulsions*:—

September 29th, 1852. Whilst still in the house of a patient who had been just delivered of her first child, after using chloroform for eight hours, I was summoned to a lady residing ten miles from Dublin, whom I had attended at the birth of three children while she had lived in town, but on this occasion, having made up her mind to remain in the country, she had placed herself under the care of a gentleman in her neighbourhood. I reached the house at four o'clock, p.m., and as I entered her room she was seized with a most violent convulsion.

Immediately I opened a vein in the arm, and took upwards of twenty ounces of blood. The convulsion subsided, but she did not recover consciousness. I was struck by the extraordinary extent to which her limbs and face were swollen, and I now learned from the gentleman in attendance that she had suffered during the last two months from a gradually increasing swelling of the legs and feet, which finally reached the thighs and upper extremities, and face. She had taken labour early in the morning, and was delivered about twelve o'clock of a very deformed child that did not live. Very soon after the expulsion of the child she complained of pain and swimming in her head, and became confused in her ideas and speech. It was on the occurrence of these symptoms I was sent for. No convulsion had taken place before the one I witnessed, but the threatening symptoms had never subsided from the commencement. As she did not recover after the bleeding, I expected she would not be long without another attack of convulsion. In this I was not disappointed, for in about a quarter of an hour she was again seized, and again I drew a similar quantity of blood from the arm. The hair was cut off, cold was applied to the head; still there was no sign of any return to consciousness on the subsidence of the fit. Tartar emetic in large doses was with difficulty got into the stomach. In less than half an hour another violent fit supervened. Her state was now one of great danger. Bleeding could not be carried any farther. The pulse was small, fast, and feeble. Chloroform appeared to hold out the only hope for her. I at once commenced its use, with most decided effect upon the convulsion. It subsided more rapidly than either of the others had done, she slept more naturally after it was over, and seemed rather less stupid when the sleep ended. Nothing like consciousness, however, appeared. I sat by the bedside and watched the advent of another fit, which being perceived I applied the chloroform and cut it short. This occurred several times, with intervals gradually prolonged between the attacks. At the end of two or three hours she opened her eyes and looked about her, though still quite unconscious and unable to answer questions. From the state of stupor she gradually passed to a condition of mania, like what I had before seen in patients recovering from severe convulsions. She sat up in bed, endeavoured to get out of it, and knew nobody around her. In this state the chloroform was of the highest benefit, for by its use she was speedily calmed down, and having kept her under its influence she soon fell off asleep. Whenever she awoke and showed signs of similar violence, the same remedy was at hand to tranquillize her; and in this manner she

was kept under the effects of the drug for twelve hours: at the end of that time she was able to recognise her friends about her, and she slowly but completely recovered.

I am not aware that the powerful agent, for which we are indebted to Dr. Simpson, has been used by any one in cases of *uterine hemorrhage*; and from the imperfect knowledge hitherto attained of the truly wonderful properties of this medicine, it is not surprising that its application to such cases should have been looked upon as inadmissible by even its most warm supporters. In the case I am about to relate, I was, I may say, compelled to resort to chloroform for assistance, and I am happy to say I did not call in vain. The success was so striking, the result so gratifying, that I did not hesitate to employ it in another and similar case, which occurred since I commenced to put these observations together for publication.

February 14, 1853. I was brought five miles from Dublin to attend a lady who had been under my care in three former confinements while she resided in the city. On my arrival I found the head just passing through the vulva, the labour having been short and easy. The child was soon expelled, and before the placenta came away a very profuse hemorrhage took place.

A drachm of ergot of rye was now administered, pressure, &c., of course, was attended to, and the placenta, being found in the vagina, was removed. The hemorrhage continued with great rapidity, notwithstanding very good contraction of the uterus. Vinegar and water, freely applied, seemed unavailing. The ground was covered with snow at the time. I ordered a bucketfull to be brought up to the room, and making up balls I passed them into the vagina, and heaped the hips and abdomen with the snow. By these means the hemorrhage was at last arrested, but the patient, a very small, slender woman, was reduced to a very low ebb indeed. The pulse was nearly imperceptible, the breathing distressed and gasping, and the formidable complaint of noises in the ears was urgently made. Fifty drops of laudanum in brandy were given, and in a quarter of an hour, the symptoms growing worse, seventy drops more and large quantities of the brandy were taken. Great exhaustion, great nervous excitement, great desire for sleep, harrassed the patient. Repose was indispensable to her safety; opium did not procure it; time was of consequence. It occurred to me that if I could tranquillize the nervous system for even a short time, the opium she had taken would come into play and continue the narcotic influence so essential to her life.

I fortunately had my chloroform with me, and as she lay tossing from side to side and calling for air, I applied the chloroform to her nose. She soon became more calm, by degrees the jactitation ceased; she assumed a more composed attitude; and, to my great delight, sleep, quiet and natural, soon came over her. Hot jars were placed to her legs and feet, and finding the sleep so natural, I held the instrument with the chloroform at a distance from her mouth, so as to keep up the action in a faint degree. It was most interesting, and, as may be well imagined, very exciting, to watch the state of the pulse during this time. I was too far off to get any assistance. I had tried an experiment with a powerful agent, but my firm belief was that the new medicine would save her life. With the finger on the pulse while she slept, I waited for the returning wave, sometimes imagining the impulse was greater, again finding it feeble as before. But it did increase in strength, and before she had slept half an hour there was a manifest improvement in the beat. The feet were kept warm, and the sleep was kept up for two hours, at the end of which time she awoke most miraculously refreshed. In fact I never saw any patient so thoroughly recovered at the end of twenty-four hours as this lady was at the end of two. She rapidly returned to perfect health.

Encouraged by the marked benefit derived from the administration of chloroform in this hitherto forbidden kind of case, I was led to employ it under similar circumstances on the 8th of the present month (March).

A lady was delivered of her third child after a natural easy labour, and as soon as the placenta came away a great rush of hemorrhage followed, which continued with great violence for an hour. Ergot and the usual means succeeded in arresting the flow of blood, but she was left in a very exhausted condition. Absence of pulse, prostration, sighing, jactitation, &c. were prominent. Laudanum in doses of fifty drops, repeated in ten minutes, and brandy freely given, failed to procure rest. Indeed the opium seemed rather to prevent sleep. I now placed a piece of sponge in the bottom of a conical shaped wine-glass, and having moistened it with chloroform, I held the glass over the mouth and nose of the patient. The medicine did not fail to produce its usual soothing effect. Soon the nervous excitement passed away, by degrees the eyelids closed, and healthy natural sleep was induced. This was maintained for two hours, during which the warmth of the body returned, the pulse returned at the wrist and slowly regained strength, and at the end of the time when she awoke she said she felt perfectly well and happy.

She described the first effects of the chloroform as being most delightful. The peace of mind and soothing of the whole nervous system produced by it, she declared resembled a foretaste of heaven.

My attention was arrested on reading, in the Dublin Medical Press of January 11, 1854, the report of the meeting of the Royal Medico-Chirurgical Society, held on the 13th of December, 1853, when Dr. R. Lee read a paper purporting to contain an account of certain cases of parturition in which chloroform was inhaled with pernicious effect. The report in the Medical Press corresponds with that given in the *Lancet* of December 24, 1853; and as no attempt has been made to contradict the truth of these reports, I am justified in supposing them to be correct. When a man is arraigned for libel in a court of justice, it is usual to read the article for which he is accused, and with your permission I will imitate that example and read the offensive document in question:—

“In these seventeen cases the author traced a series of injurious consequences to the employment of chloroform during labour. Thus, in Cases I. and II., the contractions of the uterus were arrested by the chloroform, and delivery was completed by craniotomy. In Cases III., IV., V., X., XIV., XV., and XVI., insanity and great disturbance of the brain followed its use. The necessity for delivery by the forceps was attributed to its employment in Cases VI., VIII., XI., XII, and XIII. Dangerous or fatal peritonitis or phlebitis ensued after the exhibition of chloroform in Cases VII., VIII., XI., and XIII. Epilepsy occurred in Case XIV.; and dangerous fits of syncope arose from its use in Case XVII. The reports of friends had confided many more analogous cases, and public rumour swelled the list still further, but he was desirous of confining attention to those which came directly under his own observation. He thought that a contemplation of the subtle action of this poison on the nervous system would have induced caution in its application to practice, but, on the contrary, the greatest levity had characterized its employment. Very soon after the discovery of its physiological effects, the author was astonished and confounded by the announcement of its application to midwifery; and it was not difficult for him to foresee that rashness, in its application and use, would lead to most deplorable results, and he regretted to find, that in this he had not been mistaken. It was not wonderful that women doomed to bring forth their offspring in pain and sorrow should seek to escape from the troubles of our race by means of this treacherous gift of science; neither could we feel surprise that the in-

stances of women who were saved from the grievous pains of child-bearing, without bad consequences, should have for a time reduced to silence those unwelcome monitors who pointed to the possible evils of this new agent; but it did seem strange to the author that, amidst so wide-spread an experience as now existed of the noxious and dangerous effects of chloroform, it should be necessary for him to assemble the proofs of the havoc it had made. The two most serious effects produced by chloroform on women in labour were, a languid and deficient contraction of the uterus, and a greater susceptibility to the risks that arise from inflammation and fever. With regard to the first, the direct testimony of his own senses convinced him that the action of chloroform did very manifestly slacken the uterine contractions, and in some cases had put a stop to them altogether. Of the second class of effects, the risks of the puerperal condition were much complicated; for to inflammation and fever must be added severe cerebral and nervous disorders. He had no doubt that the use of this noxious agent ought to be expelled from the practice of midwifery. In conclusion, the author observed that, though his opinions had been confirmed by conversation with the most discreet and experienced practitioners, yet he entertained grave doubts of the result of the present appeal to the good sense of the profession, when he considered the arts used to propagate a faith in this practice. It had become almost an extra-professional question. There was a systematic concealment of truth by physicians; appeals were made to the natural timidity of woman; and the most fallacious promises of perfect safety were boldly held out. Conceited or ignorant women of fashion made a pastime of this as of other quackeries, and the cause of science and humanity was placed in the hands of the most presumptuous and frivolous part of the community, while young and inexperienced mothers were decoyed to their destruction. If he had helped to rescue the medical profession from the dominion of a great and dangerous error, and had placed some restraint on an ignominious and disgraceful practice, the author would rest satisfied that this essay had not been written in vain."

As I have been amongst the earliest advocates in this country for the use of chloroform in parturition, and am, by daily experience, more and more thoroughly convinced of its immense advantages, I cannot suffer so gross an attack upon the characters of those who employ this agent to pass without making some observations on the subject, at this the first meeting of the Obstetrical Society which has taken place since I

read the report of the meeting of the Royal Medico-Chirurgical Society, at which the paper was read.

Two points are to be attended to in commenting upon this most extraordinary document:—1st. The cases by which the prejudice of the author is supposed to be bolstered up; and next, the offensive language in which sentiments and opinions equally offensive are expressed. Unfortunately, the cases have not been published, and, therefore, we are at a loss to know what value to place upon them; but, judging from the headings under which they are grouped in the published report, I think the reputation of chloroform in midwifery has little to apprehend from this attempt to extinguish it.

Thus we are told in Cases I. and II., the contractions of the uterus were arrested by the chloroform, and delivery was completed by craniotomy. Now we do not require any details to enable us to perceive the folly of adducing such cases as pernicious consequences of chloroform. Every one who has written on the use of chloroform in parturition has stated, that when given in large quantity, so as to cause insensibility, the effect, at first, is to arrest the action of the uterus; that, every one who has had real experience in the use of the drug, is well aware of, and cares little about: for he knows that in a very short time the action will return and go on as well as before. But he also knows, that if the agent is judiciously and cautiously employed in the manner I have advocated, so as to diminish sensibility to pain, without destroying consciousness, no such arrest of uterine contraction takes place, but, on the contrary, the fibres seem to act with increased vigour. But if, in these cases cited by Dr. Lee, the medicine had been used rashly and too freely at first, and a temporary suspension of uterine action had occurred, why they should eventuate in craniotomy does not appear. We all know that uterine contractions are often suspended naturally for hours in the middle of a labour, when no chloroform had been used; but that *alone* would never lead one to resort to craniotomy; there must be something else in the case, besides mere want of action in the uterus, to warrant such a proceeding; and so it must have been in these cases cited by Dr. Lee. Very likely the uterine action was interfered with by a precipitate employment of the drug; but it is also likely that the cases were such as would have required craniotomy equally, if chloroform had never been used; for I cannot for a moment imagine, that a physician of Dr. Lee's experience would resort to such an operation on the simple grounds of a temporary arrest of uterine action. In the

absence of details of these cases, we may safely put them down as *post hoc ergo propter hoc* cases, and class them among the absurd exaggerations (to use no more severe term), with which partisans so often attempt to mislead their hearers. If the cases ever occurred, they were bad cases requiring craniotomy, in which chloroform was used (perhaps rashly and unskilfully); and because the drug had been employed, the necessity for the extreme proceeding was attributed to it, when it might as well have been laid at the door of the cup of tea the patient had taken a week before. We have next seven cases in which insanity and great disturbance of the brain are stated to have followed the use of the drug.

In the absence of all clue to the nature of these cases, I can only observe, that when they are published the profession will have an opportunity of judging of their value; but, in the mean time, from a large experience in the use and effects of chloroform, I think it right to mention, that I have never met with a single instance of such results, and therefore must be excused for receiving with very great doubt any such assertion.

The necessity for delivery by the forceps was attributed to its employment in five cases. Why or wherefore does not yet appear; and I expect it will require some ingenuity to connect the effect with the supposed cause.

Dangerous or fatal peritonitis or phlebitis occurred in four cases after the exhibition of chloroform; and why should they not occur in those cases as well as in the thousands of cases in which they occur where no chloroform has been used? No one has ever said that patients who have used chloroform are to be exempt from the ordinary consequences of parturition; and unless it can be shown that a greater number of women are attacked by the diseases just mentioned, after the use of chloroform, than without it, it is absurd to adduce such instances as proofs of the "*pernicious effects*" of chloroform. One case of epilepsy, and one case of dangerous fits of syncope, make up the seventeen witnesses upon whose testimony this "treacherous gift of science," this "noxious agent, ought to be expelled from the practice of midwifery." It is no very uncommon occurrence, that an indiscreet and rash advocate should damage his cause by furnishing his antagonist with weapons calculated to defeat him. The animus of Dr. R. Lee can be gathered from the offensive sentiments expressed in the paper under consideration. It is quite plain that he exerted all his ingenuity, and adduced all the cases he could, to damage the reputation not only of chloroform, but of all who advocate its use; and yet, by this very effort, he furnishes them, on the very best

authority (that of a reckless and bitter opponent), with the grand fact, that he can adduce no instance in which a fatal result can be traced to the use of chloroform in midwifery. Where, in these seventeen cases, do we find those "most deplorable results," those "noxious and dangerous effects," that "havoc" attributed to this "treacherous gift of science"? Where, but in the brain of a partisan, wilfully shutting his eyes to the wonderful and still undeveloped powers of a new agent, with which it is quite plain he is still unacquainted, and seems determined to remain so. Leaving such exaggerated epithets to find their value with all rational observers, I cannot conclude without indignantly repelling the language applied to myself and others who have used and recommended the use of chloroform:—"There is a systematic concealment of truth by physicians;" and again, "an ignominious and disgraceful practice." Now, freedom of discussion—freedom in the fullest sense of the word, in which it can be used in polite society—I advocate and admire; without such freedom we cannot expect to arrive at satisfactory conclusions in many points of practice; but the unwarrantable license of such expressions as I have just quoted must be condemned by all, as subversive of that decorum which should attend controversy among medical gentlemen; and must be characterized as an audacious libel by those implicated in the accusation.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

A Clinical Introduction to the Practice of Auscultation and other Modes of Physical Diagnosis in Diseases of the Lungs and Heart. By H. M. HUGHES, M.D., Assistant Physician to Guy's Hospital, &c. Second Edition. London: Longmans. 1854. Post 8vo, pp. 302.

Lectures on Clinical Medicine. By JOHN HUGHES BENNETT, M.D., Professor of the Institutes of Medicine and of Clinical Medicine in the University of Edinburgh, &c. Edinburgh: Sutherland and Knox. 1850 to 1853. 8vo. Parts I. to VIII.

To form an accurate diagnosis in thoracic disease involves a synthetical as well as an analytical mental process: by the first, we determine those analogous phenomena which result from the combination of various physical conditions,—by the second, we infer the special character of these several conditions in their relation to such phenomena. The first demands a comprehensive knowledge of the anatomical and physiological characteristics of the healthy structures as the only safe foundation for a sound pathology—the second requires a close intimacy with morbid operations as they are evidenced through alterations in these structures. Pathology has therefore a twofold relation,—to disease as the exponent of vital action,—to health as the evidence of an abnormal condition. To study with advantage or practise with success, it is above all things essential we learn to appreciate these relations; the suggestions of theory will be thus schooled by the realities of experience; and the mind, led beyond the contemplation of mechanical to vital influences, will learn to ignore much of what abstract reasoning on material changes might have premised. “Hypotheses,”

Sydenham writes, "owe their origin to ostentatious vanity and idle curiosity, whence it is easy to conceive how much they must needs obstruct the improvement of physic, which is a science that depends chiefly on well-conducted experiment and close and faithful observation." Has not time but more truly confirmed this remark? Read nosological descriptions of a disease, and nothing appears more simple than its recognition, yet, absolve the mind from every preconception, and then closely study the progress of morbid action,—see alterations of structure without appreciable functional derangement,—observe functional derangement, frequently inexplicable on other supposition than the exercise of innate power by the material instrument,—and must not the great truth, whose value will be proportionate to the largeness of our conception of it, forcibly impress those who philosophically study? Medicine is a science of observation, whose principles rest neither in the adoption of those laws which regulate the changes of matter, nor yet, in the recognition of those phenomena which indicate the operations of life, but, in our appreciation of their mutual co-operation, and reciprocal relations; and, consequently, though we are enabled to speak definitely of one, yet, as we have no means of accurately estimating the other, to expect that uniformity in results which we admit as being wanting in their causes, or to infer from the mere presence of those results the particular nature of those causes, would be to adopt a process of reasoning altogether fallacious.

The elementary teacher of medicine can only propound those principles which have been deduced from admitted or demonstrable truths. He can at best but prepare the student's mind for that self-education to be acquired at the bedside of the sick, since, by informing him how, and what to observe, the path to knowledge is thereby divested of much of its difficulty and seeming obscurity. When, however, proceeding further, theory and practice join hands, the duties of the instructor take a much wider and loftier range, for in analytically treating so vast a science, beside establishing those principles and demonstrating those truths which form its basis, it is requisite he also expound the method of deducing from those principles and truths such general rules as may be subservient to others for the purposes of investigation;—while in order to prove the value to be attached to those rules or truths, it further behoves him to illustrate their application to particular examples: which preargues such a familiarity with disease as enables it to be determined in any individual case, how far coincident manifestations modify each other, and in what respect affections essentially

identical differ in their ultimate effects. Hence it is that clinical lectures must be ever regarded as the most valuable additions to medical literature, and at the same time the severest test of the physician's capability.

We have deemed it advisable to isolate such of Professor Bennett's Clinical Lectures as treat of thoracic disease, for the purpose of embracing them in our consideration of Dr. Hughes' recent work, inasmuch as we are satisfied that in the comparison of coincident writings the duty of the reviewer is often as fully accomplished as by the expression of critical opinions; while the relative value of the doctrines the works expound is thereby not unfrequently truly demonstrated. Dr. Hughes has, we are of opinion, offered to the profession a book of no ordinary value, even though wanting the charm of great originality: minute without being wearisome, comprehensive without proving prolix, every page bears the impress of the author's thorough appreciation of the subject he has undertaken to treat, with an anxious capability of fully and clearly imparting his knowledge. Written up, we might almost add, "*ad nauseam*," we scarcely believed a new work on auscultation should have proved such an acceptable addition to our existing literature, as that which, the preface informs us, Dr. Hughes has at length yielded to the urgency of professors and pupils. Whether we would be warranted in applying the same terms of commendation to the labours of Professor Bennett is an inquiry whose answer we will for the present defer; but, proceeding to lay before our readers an analysis of both works, we shall in our passing observations endeavour to afford them the fullest grounds for questioning our opinions respecting the first, and of forming their own judgment in reference to the second.

Dr. Hughes enters on the consideration of his subject by impressing pupils with the necessity of diligent and patient study in the hospital wards, where they can alone hope to educate their senses sufficiently to attain either dexterity or confidence in practice. He then proceeds to offer such cautions for the guidance of the conduct and demeanour of the examiner as we heartily approve of, among which he observes:—

"With the view of not exciting alarm in the mind of nervous individuals, all unnecessary display of stethoscope and pleximeter, and parade of every kind, should be avoided, otherwise some undefined notion of an operation may be induced, and the patient may become unwilling or unable to submit to the ordeal."

Proceeding further, we find it laid down as a principle whose propriety cannot be doubted—

“ In all examinations the benefit of the person examined should be the primary consideration.”

The student of auscultation is impressed with the necessity of a certain acquaintance with the elementary sciences of his profession, conjoined with a general knowledge of the ordinary symptoms and results of morbid processes, without which, he must be unable to appreciate various changes of structure, or rightly estimate the knowledge derivable from the previous history and general or constitutional symptoms, whose relation to the physical signs affords the only safe grounds for practical deductions.

Dr. Hughes next proceeds to point out the method to be followed in auscultatory examinations, and, to assist the student, proposes such a division of the chest into regions or compartments as will enable him to render his previous knowledge available for recognising healthy sounds, and marking with accuracy the site of any lesion. Having particularized the different modes of exploration to be adopted in diseases of the chest for the determining of *physical signs*, in contradistinction to *rational symptoms*, he proposes to treat of them under the following heads:—Inspection, Palpation, Percussion, Auscultation, Mensuration, and Succussion.

Professor Bennett's preliminary remarks on the examination of the patient, taking a much wider range, equally demand our notice and approval. Advocating most justly method as well as accuracy in the inquiry, he proposes to his class the adoption of a plan which he had learnt when a clinical student in the wards of Professor Rostan, of Paris, its object being—

“ To arrive as quickly as possible at a knowledge of the existing condition of the patient, in a way that will insure the examiner that no important organ has been overlooked or escaped notice.”

This plan, being calculated for general clinical researches, proposes a scrutiny of the circulatory, respiratory, nervous, digestive, genito-urinary, and integumentary systems, with the antecedent history, for the arriving at an accurate knowledge of his condition during life; while, for the carrying out of post-mortem examinations, we are afforded such hints as, if attended to, could not fail to render us thoroughly acquainted with even the minutest morbid change.

So far, both authors are excellent in their suggestions: as, however, we proceed, we shall find that such unanimity is by no means maintained.

Dr. Hughes' second lecture treats of “ the method to be pursued, and circumstances to be noticed, on inspecting the chest,”

as well as "the indications afforded by inspection." That inspection of the thorax is of great value as an aid to the formation of our opinion must be at once evident, since by it we are enabled to appreciate visible departures from the harmonious action indicative of health, or deviations from the normal configuration; as also, to compare the diseased and healthy side; while in some instances, we can, moreover, infer from the external appearance the nature of those internal causes giving rise to particular changes. The character of the respiratory movements in pleuritic inflammations—the almost passive condition of the ribs and the peculiar nature of the thoracic enlargement in emphysema—the widening and polished appearance of the intercostal spaces, with their elevation, which, generally speaking, may be regarded as evidence of empyema—the irregularities of surface, together with the condition of the capillary vessels witnessed in malignant disease—the flattening of the chest in phthisis—the existence of tumours or visible pulsations—together with the recognition of acquired or congenital malformations,—are enumerated by Dr. Hughes as some out of the many causes which render it essential that "in every primary examination, and, indeed, in all examinations in which an opinion is expected to be delivered as to the condition of the internal organs, Inspection should never be omitted," whose institution, if delicately managed, we agree with the author, "may be perfected without offence to the most fastidious."

The mode and advantages of employing Palpation, with the indications it affords, are discussed by Dr. Hughes in his third chapter. Its assistance to diagnosis in determining the relative mobility of the different sides; the absence or presence of vocal vibrations; in detecting frictions; in ascertaining the positions and estimating the movements of organs; in recognising tumours; in diagnosing between fluid and solid enlargements; in estimating the force, extent, and character of pulsations,—are each portrayed in a clear and practical manner.

On neither of these modes of investigation, their necessity or advantage, has Professor Bennett, in his minute directions respecting the examination of the patient, expressed an opinion.

The important subject of "Percussion, or examination by striking," occupies Dr. Hughes' fourth chapter. It is scarcely requisite to quote observations confirmatory of its value, inasmuch as we believe it is universally admitted. Having detailed the advantages to be derived from, and the precautions to be taken in, instituting immediate percussion, and stated those objections which exist to its general application, Dr.

Hughes proceeds to the consideration of mediate percussion. To the method to be followed, and cautions to be borne in mind, with the general directions this chapter contains, we freely accord our fullest approval. Percussion in the normal state of the chest, as indicating the natural resonance of its different regions; the information it affords in diseases of the lungs and bronchi, in diseases of the pleura, in diseases of the heart and pericardium, in aneurism of the large vessels, and abscesses or other diseases of the mediastinum, is distinctly treated of and fully described; the inconstant and variable nature of the sounds percussion elicits in those different affections minutely set forth; and the causes which may modify their development at the same time detailed.

Professor Bennett has devoted a section of his clinique specially to this subject; we shall, therefore, pause to contrast the observations of the two writers. Professor Bennett being silent in reference to Immediate, passes at once to the consideration of "Mediate, percussion;" and advocates, for this purpose, the use of the ivory pleximeter of M. Piorry, as modified by M. Malliot, in conjunction with the hammer of Dr. Winterich, of Wurzburg. We have already given expression to Dr. Hughes' suggestions respecting the employment of these instruments. The advantages Professor Bennett attributes to their use are ranged by him under three heads, viz.:—

"1st. That the tone produced, in its clearness, penetrativeness, and quality, far surpasses that which the most practised percussor is able to occasion by other means. 2nd. It is especially useful in clinical instruction, as the most distant student is enabled to distinguish the varieties of tone with the greatest ease. 3rd. It at once enables those to percuss who, from peculiar formation of the fingers, want of opportunity, time, practice, &c., are deficient in the necessary dexterity."

Dr. Hughes seems to be sadly blind to those many merits, for he, when fairly estimating the advantages of such instruments, objects to their employment on many grounds, one, amongst others, being that—

"Each participates more or less in the objection, of itself giving rise, when struck, to a sound which interferes with that dependent on the vibration of the thoracic parietes."

Again we read,—

"The very best *pleximeter* is produced by one of the fingers of the left hand, and the very best *percussor* is produced by the ap-

pressed fingers of the right hand. They possess the advantages of all others, and some which are peculiarly their own."

Contrasting this with the first of Professor Bennett's propositions, have we not a complete antagonism in the views of the two writers? Of which are we to approve? We believe the recommendations and objections of Dr. Hughes are impartial and scientifically correct; they are as follows, when speaking of the use of the finger as a *pleximeter*:—

"It is not open to the objection of sometimes causing pain by firm pressure upon fat or thin persons, as are the wood or ivory discs which are occasionally employed. It is capable of being much more nicely adjusted to uneven surfaces than either those rigid substances, or stiff leather, and can be much more easily maintained in the required situation than flexible but elastic India rubber. In thin persons the intercostal spaces form hollows which cannot be filled by unyielding pleximeters; air constantly intervenes between them and the parietes, and by its resonance, when the pleximeter is struck, modifies the sound dependent on the vibrations of the instrument itself, and thereby adds to the sources of fallacy thence arising. The stiff pleximeters, moreover, cannot be conveniently applied in the axillæ, or, in very thin persons, above or immediately below the clavicles. The firm pressure of the fingers, on the contrary, causes no pain; they are capable of being perfectly adapted to the surface of the chest however uneven; they are therefore equally efficient as pleximeters in fat and thin persons; they can be applied in the hollow of the intercostal spaces; they can be employed in every part of the chest; when struck they give rise to very little independent sound, and they are always at hand ready for use."

On this point, without much apprehension, we leave the decision to the common sense as well as to the experience of our readers. Next then in reference to Professor Bennett's second reason, "It is especially useful in clinical instruction, as the most distant student is enabled to distinguish the varieties of tone with the greatest ease," we would ask, is the patient benefited, or the most distant student in reality instructed by such hammering for knowledge? The one, we believe, may be rendered both nervous and excited; and the other, far from being advantaged, gains, at least, but an imperfect estimate of the truth; while, again, in many cases, we believe, with Dr. Hughes, that this loudness of sound is anything but desirable or trustworthy, since, in some instances, "a gentle tap will often display a marked difference in the two sides, when a strong blow will be utterly inoperative for the purpose." The reasons included under the third head are so futile, that they are positively unworthy serious consi-

deration, if advanced as scientific objections;—such a peculiar formation of fingers as Professor Bennett has twice met with; we imagine may exist about once in a thousand students. We deny that knowledge worth anything can, as Professor Bennett would lead us to believe, be acquired intuitively from the use of such instruments by those who, “from want of opportunity, time, practice, &c.”—(in the “&c.” we presume Professor Bennett includes all else essential to such information)—“are deficient in the necessary dexterity;”—since it is this necessary dexterity which lends either value or credit to the sounds elicited; and it is this opportunity, and the experience it affords, which alone enables those sounds to be properly distinguished or rightly estimated.

Professor Bennett, having thus satisfactorily settled the advantages offered by his favourite instruments, proceeds to notice the different sounds produced by percussion. The nine elementary sounds of M. Piorry are by him embraced in three divisions, viz.:—the tympanitic, the humoral, and the parenchymatous, according as they proceed from the organs containing air or fluid, or the tones being formed of a dense uniform parenchymatous tissue throughout.

“The sense of resistance,” writes Professor Bennett, “should be as much educated by the physician as the sense of hearing, and it would be difficult for an individual practised in the art of percussion to say which of these two points is the more valuable to him. Both are only to be learnt by practice, and considering it perfectly useless to describe that in words which may be learnt in half an hour by the use of the pleximeter and hammer on a dead body or the living subject,” &c. &c.

Hear, in reply, what Dr. Hughes thinks:

“The practice of this mode of exploration, apparently so simple, requires great nicety, tact, and delicacy, and considerable experience for its efficient application, &c. &c.”

It appears to us, that, according to Professor Bennett, it is a matter of no moment whether the student learn to percuss, or to estimate the sense of resistance, on the dead or living subject. Now, we consider it makes all the difference conceivable. We are satisfied it is both unsafe and untrue to promulgate the doctrine, that familiarity with disease can be so easily acquired, since vague and unsatisfactory results must ever follow hasty and imperfect generalization. Much time and anxious bedside study, with patient self-teaching, can alone impart that capability deserving confidence. The sense of touch must be educated; its powers of appreciation refined;

its useful capacity developed, if not formed, before those fine differences in tone or quality associated with the sensations conveyed on percussion or palpation, can be truly estimated. We do not hesitate to say, no half-hour's teaching could give this. If Professor Bennett, by these observations, means to convey that pupils, by such a prolonged investigation, may satisfy themselves of the fact universally known, and, we believe, generally admitted, that, proportionate to the volume and density of the body struck is the tone elicited from it, or the resistance afforded by it, we decidedly assent, that five minutes, aye, even one minute, would be sufficient for such a purpose: but we have no hesitation in declaring, that the useful application of such knowledge for the examination of disease demands its continued and careful exercise on the living body, where the capability of estimating vital forces can alone be acquired.

We seek not to deny that examinations may, in certain exceptional causes, be facilitated by the employment of the ivory pleximeter; but our views respecting its general adoption coincide with those Dr. Hughes so justly advocates. We conceive that, on scientific grounds, the use of the finger is to be preferred, since by it we are enabled, while estimating the difference in sound, to also judge of the character of the resistance and different degrees of resiliency in the organs examined. We believe, however skilfully employed, the pleximeter, for such a purpose, would never equally respond to our expectations. We cannot, therefore, avoid expressing our opinion, that in his advocacy of his favourite instrument, Professor Bennett has manifested an exclusiveness which the experience of Dr. Hughes, and that of many very distinguished European teachers, by no means corroborate.

In his fifth chapter, we are brought by Dr. Hughes to the study of auscultation. Professor Bennett has also given a section specially to the same. The subject is so extensive, and embraces so many points of interest, that to attempt doing full justice to all Dr. Hughes' observations would require a closer analysis than our space could at present admit; we shall, however, while being of necessity brief, endeavour to present our readers with a few, that they may anticipate the many excellent practical remarks the perusal of this work will afford.

Premising that the direct application of the ear to the surface is not only fully as effective as mediate auscultation in some, and that in many cases, it is even more so, directions are given respecting its employment; while the study of me-

diate auscultation, being that demanding the greater attention, to it Dr. Hughes' chief remarks are directed.

The several varieties of stethoscopes which have been proposed, and the principles which are considered to regulate their efficiency, are fully set forth,—the instrument Dr. Hughes most approves of being that formed of wood such as is ordinarily in use. Having detailed the various forms of this instrument, we read—"The form of the instrument is of very trivial importance; and that the best stethoscope for any individual is clearly that with which he can hear best—which, by experience, he finds best suited to his ear." Professor Bennett advocates the employment of those made of gutta percha. Speaking from our own experience, and reasoning on the method in which the sound is propagated, we may state that we gladly abandoned the use of instruments formed from that material; and we do not believe, that even in the most experienced hands they will ever fulfil the purposes for which they are designed, equally well as those Dr. Hughes recommends.

In their directions respecting the mode of using the stethoscope, Dr. Hughes and Professor Bennett are equally explicit. In very thin patients Dr. Hughes advocates the intervention of a pad of soft linen between the surface of the thorax and bell of the instrument, or to disregard the stethoscope and use the ear. Attention is also directed to the liability which exists to confound the creak produced by pressing the bell of the instrument on an œdematous surface, with sounds either originating in the lung, or dependent on conditions of the pleura.

In his observations on natural respiration, the different characters of the sounds in various situations and regions of the chest are enumerated, and the causes which may modify those natural phenomena particularized. Alluding to the indications from the voice, the difference between perfect and imperfect pectoriloquy is set forth, and both contrasted with bronchophony, while the causes which may modify and assimilate these tones are, we conceive, justly reasoned on. We pass many valuable observations on the character of the dry sounds in diseases of the larynx and trachea, and affections of the bronchial tubes, to pause on the author's remarks in reference to the nature of the respiratory sounds in emphysema. Dr. Hughes regards the prolongation of the respiratory murmur to depend partly on the diminished elasticity of the ribs, consequent on long-continued pressure exerted on them by the

enlarged lungs from within, and partly from the inability of the compressed air within the lung to escape through the absolutely or comparatively contracted bronchial tubes, with a rapidity equal to that with which the ribs descend. In his observations respecting the respiratory phenomena in phthisis, the true value of tubular, amphoric, and fistular breathing is canvassed. Metallic respiration being nearly allied to the two last, the difference of their causes is investigated and described by Dr. Hughes to be as follows:—

“The difference of the fistulous and the amphoric respiration depends upon the size and form of the opening leading into the cavity of the lung or pleura; that the opening is small or obstructed when the former, and comparatively large when the latter sound is heard, and therefore the amphoric respiration may become fistulous by the tube through which the air gains admission into the free space becoming obstructed with secretion, or being diminished in its caliber from any other cause, and that fistulous may be converted into amphoric breathing by the removal of such obstruction, or by the tube from other causes becoming enlarged. I conceive also that the metallic ringing of the respiration is dependent rather upon the nature of the walls of the cavity itself than upon the form or size of the opening leading to it, and that it is from the vibration of these walls being communicated to the air contained within them that the peculiarity of the tone arises.”

In his notice of the auscultatory phenomena, indicative of affections of the pleura, amongst other practical observations we find the occurrence of tubular breathing and shrill vocal resonance coexisting with a pleura full of fluid. This apparent anomaly Dr. Hughes explains by the supposition that, admitting the pressure of the fluid is not sufficient to wholly compress the larger bronchial tubes,—

“The vibration of these tubes is communicated to the spine, or to the bony parietes, against which they rest, and thence, as in a well-braced drum, to the walls of the entire cavity,—the fluid being so pressed that it moves as a solid, and conveys the vibrations of sound tightly as a solid to, and with, the solid walls.”

The moist sounds in diseases of the air-passages are next passed in review. We shall rest satisfied with observing that Dr. Hughes, in his reasoning on the nature of those purely physical conditions adequate to their production, fully establishes the variety of circumstances which may occasion their presence, and perfectly negatives the supposition of any necessary association between the various physical conditions adequate to their production and the existence of a particular

constitutional state. Professor Bennett offers to his class "a condensed resume" of those sounds. It most assuredly is as condensed as it possibly could be. It may be the very essence of knowledge, small in volume but great in strength, yet to us the information appears to have been dealt with rather a sparing hand, since this "condensed resume," though sufficient to direct the learner in the few particular cases subsequently detailed, would scarcely enable him to undertake observations of his own. It is, however, satisfactory to know, that both these writers coincide in their conclusions, for while Dr. Hughes declares "physical signs are indicative only of certain physical conditions, and not of particular diseases," Professor Bennett adds: "The different sounds are only indicative of certain physical conditions of the lung, and in themselves bear no fixed relation to the so-called diseases of systematic writers."

Auscultation of the heart next receives Dr. Hughes' attention:—The healthy character of its impulse, rhythm, and sounds, with the natural range of these phenomena, are separately noted, as also the peculiarity of the several modifications they undergo in disease. The varieties of murmur and the different nature of the causes capable of producing them, together with the grounds on which the diagnosis of exocardial and endocardial sounds rest, receive from Dr. Hughes the same close and impartial attention. Professor Bennett briefly alludes to these phenomena, and concludes his observations by the very practical observation—"Their true diagnostic value can only be learned by the careful examination of individual cases."

Dr. Hughes, in his sixth and seventh chapters, discusses the advantages of mensuration and succussion. Under the first, the assistance to be derived from the employment of the spirometer, chest measurer, and stethometer, are thus set forth:—"I cannot help expressing my belief that to the practical physician at the bedside, they are but imperfect substitutes for experience in the investigation of the nature of existing disease; that, even to those familiar with their use, they are as crutches to the feeble, which should be discarded as he gains strength; and that, to the vigorous and active, they are but as clogs and impediments to his free action and progress." Professor Bennett thinks otherwise, for he advocates their employment, and declares that the extent of the thoracic expansion "is now capable of being accurately determined by the chest measurer of Dr. Sibson, or the stethometer of Dr. R. Quain; and adds, in reference to Dr. Sibson's chest measurer:—"My own experience of this mode of investigation has hitherto been too limited to enable me to speak confidently with regard to the value of

his method in diagnosis." The spirometer introduced by Dr. Hutchinson stands in a less fortunate position, for of it we read:—"As a means of diagnosis, I have never been able to satisfy myself of its utility." For our own part we have already expressed our opinions on the value of these instruments in some late Numbers of our Journal, and they altogether coincide with those of Dr. Hughes.

The extent to which our examination of Dr. Hughes' work has insensibly reached urges us to conclude—we will not say our labours, for to read a good book comes not within such a term, but—our imperfect analysis. Embodying the existing state of our knowledge, and alike free from dogmatism or assumption, we recognise in its precepts the efforts of a practical physician, fully competent, and equally anxious, to forward science by dispassionately discussing truth. From the days of Laennec downwards, authors have vied with each other in adding to the certainty of our means of physical diagnosis. We might, with the greatest confidence, refer to the pages of this Journal, and quote many original observations which have so well contributed to dissipate the obscurity, unravel the intricacy, and establish the efficacy of symptoms, physical signs, and treatment. We are not, however, called upon to do so; yet we may be permitted to observe that, nearly twenty years ago, Dr. William Stokes thus wrote^a: "It is never to be forgotten that, although in these various classes we have a vast number of well-marked and essentially different physical phenomena, there is not one of them which, taken singly, can be considered as a pathognomonic sign. Nay, we might go further, and declare that *no possible combination of them can be considered absolutely pathognomonic.*" Our knowledge of disease since then has certainly made some progress, and it cannot, therefore, be denied, that no greater tribute could be paid to this physician's fidelity than that, after the lapse of such a time, an observer of Professor Bennett's great originality and vast opportunities should confirm the same by declaring: "No single acoustic sign, or combination of signs, is invariably pathognomonic of any certain pathological state; and conversely, there is no pathological state which is invariably accompanied by any series of physical signs."

The first of Professor Bennett's Clinical Lectures which we shall notice separately is that on bronchitis. In his preliminary observations we read:—

"It may be well, before proceeding to comment on individual

^a Stokes, Diseases of the Chest, p. 15.

diseases of the lungs, to state shortly the diagnostic general rules which have resulted from previous accurate investigations into their physical signs and morbid anatomy."

Those general rules are accordingly set forward in nine propositions, associating certain physical signs with particular conditions, as a guide to diagnosis; which signs, Dr. Hughes' observations and our own experience attest, may or may not indicate the conditions to which, in these propositions, they have been assigned. Professor Bennett admits as much, and rightly impresses the necessity of their observation being combined with the study of the concomitant symptoms. Certainly, between M. Piorry's ivory pleximeter, as modified by M. Mailloit,—Dr. Winterich's, of Wurzburg, hammer,—Dr. Sibson's chest measurer,—Dr. R. Quain's stethometer, in addition to the gutta percha stethoscope and other means,—it is determined to leave nothing untried which may appear to guide or assist the formation of an opinion. The following case cannot, however, be received as a very flattering illustration of the results which have followed on such a formidable array of means to an end. We shall quote it at length, as a full, true, and particular account of the history, physical signs, symptoms, progress, and treatment, in a selected example of a highly dangerous though by no means uncommon disease.

"*CASE I. Acute Bronchitis. History.*—Catherine Mulvie, aged 21, a servant, admitted into the clinical ward, July 21, 1851. She states, that two weeks ago, when in a state of perspiration, she took a bath in the open sea. The same evening she was attacked with rigors and other febrile symptoms; and on the next day there was dry cough, difficulty of breathing, and a sense of oppression in the chest. The cough has continued since, with more or less expectoration, but the febrile symptoms have abated.

"*Pulmonary Signs.*—On percussion, there is no dulness anywhere. On auscultation, there is harshness of the inspiratory murmur anteriorly; and posteriorly and inferiorly, on the same side, coarse crepitation.

"*Concomitant Symptoms.*—There is frequent cough, with slight muco-purulent expectoration; general debility; headach; soreness in the limbs; occasional palpitations of the heart, the sounds of which organ, however, are healthy. Pulse 62, full. Digestive and genito-urinary systems normal.

"*Progress of the Case.*—Under the use of antimonials with opiates, followed by expectorants, the pulmonary symptoms rapidly diminished. On the 25th, the respiratory murmurs on the right side were healthy, and she was dismissed."

The second case is an illustration of chronic bronchitis,

where death occurred from the supervention of acute peritonitis. Its particulars are given with equal perspicuity and brevity.

The third case, headed "*Chronic Bronchitis; Emphysema; Acute Laryngitis*," has, as its chief attraction in the fact, that, for the cure of the laryngitis, the local application of a strong solution of the nitrate of silver was successfully employed.

In the clinical observations on these cases, we are informed: the first was evidently an uncomplicated case of acute bronchitis, which had nearly run its course before admission,—that acute bronchitis may be epidemic, and constitute what is called *influenza*,—that it may follow or precede a similar lesion in the lining membrane of the nasal passages, that is, *coryza*,—that frequent attacks of an acute nature frequently eventuate in emphysema,—that "bronchitis, therefore, is an affection which the judicious practitioner will do all in his power to check and prevent." The treatment for this formidable disease we shall give in the Professor's words:—

"To check the progress of an acute bronchitis or coryza, no remedy seems so good as taking a full dose of morphia on the first, or, at the latest, second night on going to bed. In the morning, the patient should breakfast in bed, and keep himself warm at home during the next day. . . . Should, unfortunately, the disease progress, patience is, perhaps, the best remedy. But, if the bronchi become clogged, sudorifics and expectorants, especially ipecacuanha, will be useful; and a blister will sometimes dissipate any lingering trace of the disease."

In the second case, in which there was strong presumptive evidence that ventral or intestinal ulceration existed, the chief information we get of the treatment is contained in the following extract:—

"Her principal complaint, however, was the epigastric pain, which, notwithstanding the application of leeches, warm fomentations, opiates, and counter-irritants, continued to increase."

The third case, being scarcely more explicit, calls for no particular observation. We have sought in vain for even the faintest claim to commendation in these remarks, alike deficient in novelty or profundity. In treating of such a disease in an elementary manner, to have noticed each of its important bearings might have been impossible;—is, however, the omission of them altogether excusable? We confess our utter inability to discover from these remarks in what respect they have advanced our knowledge on that affection to which they allude, but of which they can scarcely be said to treat. If a

work in medicine can have one fault, which more closely than another approximates the inculcation of error, it is the imperfect teaching of truth. Of this latter, we may now most justly complain, and if we do not accuse the author of the former, it is not that we by any means consent to his problematical doctrines, but because we freely accord to him the same freedom in the warm advocacy of his favourite views, so long as they do not endanger life, as we reserve to ourselves in the expression of our critical opinions.

Professor Bennett's clinical observations on pneumonia next demand our scrutiny. The examples enumerated are of such a character as abound in the wards of every hospital and the case-books of most students, while the observations in reference to them are few and meagre. The occasional difficulties which arise in the diagnosis of pneumonia are illustrated by cases in which the nature of the complication preoccupied the attention and masked the symptoms indicative of that disease. In his objections to the old practice of bleeding Professor Bennett but reiterates what the late Professor Graves^a laid down when he wrote—"I may be permitted to express my doubts whether pneumonia be a disease which demands the heroic use of the lancet," at the same time that he pointed out those cases in which venesection might be useful, and the method in which it should be performed. Dr. Stokes, in his work on Diseases of the Chest, had also expressed similar opinions. Professor Bennett thus writes:—

"I place my chief reliance in the treatment of pneumonia, when hepatization has taken place, on the combined use of tartar emetic and opium, in large and frequently repeated doses."

While further we read—

"I have never yet been able to satisfy myself, that calomel favours the absorption of the exudation in cases of pneumonia; but, from the very confident manner in which this remedy is employed by practitioners of great experience, it would be presumption in me to oppose its employment. On the contrary, whenever the exudation does not disappear in the usual and natural manner, I would recommend its use, although I am inclined to think, that, if the disease be properly managed from the beginning, such cases will be rare."

Whether are we to regard this avowal as a striking instance of magnanimity or weakness of mind, where human life is the stake to be lost or won? Professor Bennett does not believe

^a Graves' Clinical Medicine, by Neligan, vol. ii. p. 42.

calomel favours the absorption of the exudation in pneumonia, and yet he recommends it for such a purpose,—he has no confidence whatsoever in its exhibition, and yet he countenances its use. It is a happy thing for the recovery of his patients the Professor has in his complacency such a safety-valve for his infidelity that he is thus occasionally induced to fire at random, not knowing where a chance shot may hit. Has not Professor Bennett met with cases in which the antimonial treatment was altogether inadmissible? In cases of typhoid pneumonia, of which Dr. Stokes^a thus writes, when contrasting the treatment by tartar emetic with that by mercury:—"The mercurial treatment is to be preferred from its greater safety, and in this disease, more than equal efficacy." "The longer pneumonia has lasted"—observes Professor Graves—"the less likely we are to derive benefit from tartar emetic, and, consequently, in most of the cases which are accompanied by decided hepatization, you observe that we prefer moderate but repeated doses of calomel, until the mouth is distinctly but not severely affected." To this we might add the following remark from the same authority in reference to the efficiency of this remedy:—"Now I believe every practical man is aware that mercury is one of the best remedies we can employ in many cases of acute and sub-acute bronchitis." Opinions such as these, and of many others of high distinction, are not to be thought lightly of. Professor Bennett's humility is not without its consolation, for, most assuredly, if in the administration of such a remedy he errs, he does so in the best possible company.

We are happily not again required to express our opinions respecting the author's Protean Lectures on 'Pulmonary Tuberculosis.' We have already in our last Number done so fully, and accorded to them that merit which a just regard for truth would permit; they are here presented to us in a much more limited form, and on their reperusal we see no just reason to revoke that judgment, to it we accordingly refer. We shall not for the present proceed further with our critical analysis of this work, but reserve for a future occasion our review of the separate cases therein recorded, as also of the chapter devoted to diseases of the heart. In conclusion, we may observe, it is ever to us a most ungrateful task to lightly estimate the labours of another:—could we on the present occasion have followed the dictates of mere personal inclination, we should have permitted Professor Bennett's clinique to pass with a brief notice, for we cannot but regret that from such vast opportunities something

^a *Op. cit.*, p. 344.

more worthy has not sprung. Great positions entail equal responsibilities. The Professor of the Institutes of Medicine and of Clinical Medicine in the University of Edinburgh being for the time identified with the world-wide renown of its School, stands forth in that light which many illustrious names reflect. The honour and reputation they have founded being intrusted to his anxious guardianship and fostering care, it behoves *him* more especially to rightly estimate that treasure it is his privilege to hold. From his clinical lectures is the profession to infer the tone and status of medical science as it exists in the University he represents. Every observation from an author occupying such a position carries in consequence a certain weight, being armed, as it were, with the authority of office; when therefore we are presented with the matured results of his continued observation, we are fully warranted in expecting something more than a few every-day accounts of disease, accompanied by such criticisms as those we have noticed, whose chief recommendation rests in their being within the comprehension of the most limited capacity. Contrast his lectures which we have reviewed with others on the same subject which have within the last few years emanated from the English, Continental, American, and Irish schools, and must it not at once be conceded, the information herein afforded is but as a drop in the great ocean of their truth?

Anteckningar om de fornämsta Medicinska Skolorna uti Italien, Frankrike, Holland, och England. Af PROF. C. SANTESSON. Hygiea, August and September, 1853. Stockholm: C. E. Fritze.

Sketches of the principal Medical Schools in Italy, France, Holland, and England. By PROFESSOR CARL SANTESSON.

WE hasten to fulfil the promise made in our last Number by giving a brief survey of the concluding portions of Professor Santesson's description of his medical tour. As we mentioned in our former notice, the numbers of the Hygiea we received while it was passing through the press contain the Professor's account of the medical schools of England, and of his visits to Scotland and Ireland.

The first in point both of antiquity and size of the medical schools of London is that attached under the denomination of "St. Bartholomew's College" to the hospital of the same name, the oldest institution in the English metropolis for the care of

the sick poor. Into the history and description of these establishments the author enters at length, tracing the gradual development of the complete medical school at present existing therein; but, as information on these points is easily accessible to our readers, we shall with more profit pass to the consideration of his general remarks. The following comparison of the working of the voluntary system, if we may so term it, which obtains in England, with the effects of the State provision made in Sweden for medical instruction, appears to us worthy of being quoted:—

“It thus appears that the course of medical education in England is expensive, and it seems particularly so to us who are accustomed to get all instruction of that kind almost for nothing, for the fees for dissection and practical chemistry in this country are so small that they scarcely deserve mention. The cause of this is, evidently, that in Sweden the State supports the schools, while those in England are, as private institutions, self-supporting; in other words, they are dependent on the fees paid by the pupils. But if it is thus considerably more expensive to pass through the medical course of instruction in England, medical practice, generally speaking, yields a proportionably greater income in that country than with us. The financial difference is thus in a great measure balanced, but a circumstance which most nearly concerns the schools deserves to be alluded to in connexion with this point, at least, in my opinion, it gives English medical teaching an advantage over ours, in which there is nothing of the kind. The circumstance I allude to is, that as in England the existence and stability of the schools depend, principally, on the fees received from the pupils, it must be the teachers’ interest to use their utmost exertions to make the instruction as good and complete as possible, and thus to attract a great number of students to themselves. Hence arises a constant stimulus to labour zealously and indefatigably in science and for the maintenance of the school, an impulse which does not exist among us, and the want of which is not unfrequently attended with evident and serious consequences. A school in England in which one or more of the teachers should show indolence, indifference, or negligence in the discharge of their duties, must inevitably fail for want of pupils and consequent want of income. In Sweden, a teacher may, when he has once secured his office and his pay, take the matter as quietly and coolly as he pleases; he may do much, or little, or nothing; he may have the largest audience, or no hearers; he may slumber soundly enough without fear of being awoke or disturbed by the superin-

tendent committee; in any case he has his given reward, neither more nor less. It is true, we might suppose that the teachers' interest in their subject should be, at least with most of them, strong enough to prevent these evil consequences; but still the possibility of the contrary is admitted, and the reality is near enough at hand,—at least where youthful and warm zeal have disappeared with years, cares have accumulated, and oftentimes economic troubles compel the father of a family to devote his time and his abilities chiefly to a very different subject from that or those in which he is a teacher. We must not always judge so harshly, but we may always assert without injustice that the students and the teaching are what suffer earliest and most from such circumstances. Besides, matters can never be otherwise in a land where instruction is privileged and limited to a few public schools authorized by the Government. In England, the Government has left instruction itself free, but has retained the final right of trial, which it exercises by certain public boards of examiners,—an arrangement which seems to be the most natural, and, at the same time, the most advantageous."

The author, having passed in review the several medical schools in London, whether directly attached to an hospital or not, observes, that the most striking points connected with medical education in the United Kingdom are, the great number of licensing bodies, and the want of uniformity in medical instruction required by them. He also remarks, as a most singular inconsistency, that individuals who hold the same qualification from the same corporation should not have the same privileges, as is the case with the licentiates and extra-licentiates of the College of Physicians of London; and that the graduates of Oxford and Cambridge have no right to practise in the metropolis or within seven miles thereof, while they are entitled to do so beyond that circuit. He also points out a great defect which exists in the non-requirement by some of the corporations of proof of preliminary general education, "*Studia, quæ si non faciant medicum, aptiorem tamen medicinæ faciunt*"—a department in which, he says, his fatherland is not deficient, as the preparatory examination required there might, on the contrary, serve as an example, and would be perfect were some addition made to it in the physical sciences; in other respects, he observes, it appears, if properly carried out, well adapted to answer its purpose.

"Another fault which has been found with the English system of examination is, that although it is received as a principle that teachers cannot be examiners except in subjects

distinct from that they lecture on, it nevertheless has happened that many members of the examining board have become, from age, indolence, or indifference, so stereotyped in their mode of examination and in their questions, that both questions and answers are known beforehand, as in a catechism. This has of late years given rise to a peculiar occupation, previously unknown in England, but much more known and exercised on the Continent, and also with us, and which consists in a person in the quality of private teacher undertaking, for a certain and generally tolerably high fee, to indoctrinate the candidate in a very short time in what he need know in order to answer the questions of such and such examiners, and so be approved, or possibly complimented, on his examination. How inimical to all real study, not to say honour and honesty, this so-called cramming system is, we know, alas! from our own experience. In England, where it has obtained the name of ‘the grinding system,’ it is more new-fashioned; but it was early exposed, and made the subject of severe disapprobation in numerous articles in the medical journals. It is principally in preparing for the Apothecaries’ Hall that this discreditable fact has been observed; and there is reason to believe that the existence of this system, together with various other circumstances, will, probably ere long, give rise to a new and better plan of medical examination, and to the limitation of the excessive number of qualification-giving corporations.”

As to the mode of instruction in the various schools, Professor Santesson expresses himself in the following terms:—“I cannot do otherwise than praise and recommend the plan adopted, especially in the so-called colleges, and which I have above more accurately described, as it is carried out at St. Bartholomew’s. The daily intercourse between many of the teachers and the pupils, the free access, and ever present opportunity the latter have of asking the advice of and of conversing with the former, meeting them as they do every day, and in many instances several times a day (in the lecture-rooms, the museums, the library, the reading-room, or at the dinner-table), and this in a manner which shows that the teacher is like a father or an elder brother to the class,—kindness, sympathy, and openness on the one side, esteem and confidence on the other,—all contribute to produce a mutual co-operation, as well between teachers and pupils, as between the latter among themselves; a system of reciprocal instruction on a great scale, which cannot do otherwise than exercise a beneficial influence both on their scientific and moral education. The adoption of examinations and oral discussions, not merely at the cliniques,

but likewise in connexion with the public lectures (once every, or every second, week, according to the nature of the subject), I consider to be good, and worthy of imitation. How different is not all this from what we generally find to obtain at most of the continental medical schools, both in France and at many places in Germany! How strange to one another are there, the teachers and the pupils,—the former most frequently neither know the latter by name nor by appearance; have seldom, in some instances never, exchanged a word with them, never addressed them otherwise than in a body and officially from the chair. The pupil sees in the teacher a person with whom he will take care to have as little as possible to do, and from whom, particularly at the examination, he will endeavour to escape as cheaply as he can. If he stand in need of advice or explanation on any subject, the teacher is often the very last he has recourse to, if he applies to him at all. To all this the English schools present the finest and happiest contrast I have found in any country.

“A stranger and guest at such a school in England should recollect not to be precipitate in his judgment on persons and circumstances. It requires time, and often perseverance, to fall in with the ways of both. This is especially true if he present himself without an introduction, and, so to speak, breaks his own way. It is, above all, necessary to be able to use the language with some freedom. In few countries ought a good letter of introduction to be of more value than in England; it saves time, and renders business pleasant. The fact is, that an Englishman, generally speaking, when a stranger addresses him, must see and examine well with whom he has to do, before he pays him closer attention, and, still more, before he shares with him his own knowledge and experience. He is kind, he is ready to oblige, but is always more or less distant, until his visitor has made some little acquaintance with him, and thus shown what he may possess calculated to awaken his sympathy and interest. Yet it is possible for a stranger to work his own way, if he has anything to communicate; but, as I have observed, it requires more time and opportunity for conversation than is necessary to a person provided with a few lines, or even a visiting card, from a friend or acquaintance. Another circumstance which should likewise prevent a stranger from passing a categorical judgment from a first impression (which is not always the most favourable) is, that the plan and mode of communicating instruction he finds here are so unlike what we are accustomed to in other countries, that to many they seem strange and almost school-like. However,

wait a little; see the result, and judge accordingly. What I can say from my own experience is, that the professional instruction in certain of the medical schools in England, and in some of the allied ones in Scotland and Ireland, is the best I have anywhere found."

The author next passes to Scotland, and afterwards to Ireland. His observations on the clinical teaching in Dublin are highly eulogistic, and he speaks in grateful terms of his reception in that city. We think it, however, more becoming, as Irish journalists, to rest content with the extracts we have given from his observations on the medical institutions of Great Britain and Ireland at large, than to quote the flattering testimony he has borne to the labours of the profession in this particular portion of the United Kingdom.

Remarks on the Examination of Recruits, intended for the use of young Medical Officers on entering the Army. By H. H. MASSEY, A.B., M.B., 4th Light Dragoons. London: Churchill. 1854. 8vo, pp. 131.

IN the title-page of this work, Dr. Massey quotes the following words of the Duke of Wellington,—words which should be borne in mind by every medical officer intrusted with the examination of recruits:—

"It must be always remembered," says this illustrious commander, "that the power of the greatest armies depends upon what the individual soldier is capable of doing and bearing." This is, indeed, the point to be regarded, for unless the materiel of which any body is composed be in its integral particles sound and of good quality, put what polish or appearance you will upon the surface, the base will still remain unsound, and unfit for hard work or rough usage. To an army the same may be applied, and here, as the selection or rejection of that base rests with the medical officer himself, the greatest discretion is necessary in passing recruits into the service, and the military surgeon or civilian intrusted with this duty should, before undertaking it, make himself, as far as possible, acquainted with the points essential in the soldier.

"The best instructor," our author observes, "is practical experience, but as this cannot be available for all, the only other source of information is the published experience of others." Dr. Massey has taken up the consideration of this subject at an opportune period, for, judging from the state of Europe at the present moment, we may well infer that we are

beginning a contest which will not soon be over, and if this be so, then the calls upon the country for recruits to fill the vacancies in the ranks, and to augment our army, will be far greater than they have been for many years. We are glad, therefore, that our author has thus devoted his time; and though we cannot say that the style of composition, or the subject matter of the volume, is altogether free from fault, or brought forward as clearly and concisely as they might have been, still we feel pleasure in noticing the work, and introducing it to our readers.

These "Remarks" are intended as a practical commentary on the War Office "Instructions" for the examination of recruits, and, as such, follow the order therein set down, and embrace the consideration, firstly, of the qualifications necessary for the soldier; and secondly, the disabilities which render a man unfitted for the service.

Under the former are included age, physical development, and physical proportion, with general remarks on selection. Under the latter, special affections of the senses, the head, the thorax, the abdomen, and extremities.

We cannot, in a cursory notice of the subject, do more than allude to one or two of these points, and we select those which we deem to be of the most vital importance,—viz., age and development.

As regards the first, we think our author has gone too far in advocating early enlistment. He says—

"As the fatigue of marching was to the infantry soldier one of his most debilitating duties in youth, it was a material objection to early enlistment; the baneful effect of sustaining too early in life the weight and drag of the knapsack on the chest on long marches, is now in a great measure obviated by the very general system of transporting soldiers on home service by railway. Night duty on guard is enumerated as an objection to young men's enlistment, since the enervating effects of want of rest in youth are fully admitted, and always deserve the attention of those who have the power of interference. In time of peace on home service, however, this objection seems over-estimated. Soldiers in some garrisons are occasionally hard worked; but this is, I believe, usually accidental, and is certainly the exception. The improvements in discipline, the judicious restraints, the habits of obedience, cleanliness, and systems, at the present day inculcated, are more readily received and permanently retained in young minds than in those of older growth, which have possibly already acquired habits inconsistent with the duties of a soldier. The much greater prevalence of temperance than formerly has likewise diminished a fearful source of temptation to lads too easily led into the insidious vice of intoxication.

Depression of spirits, or occasionally even recklessness, said at one time to be induced by the hopeless prospect of being bound for twenty-one and twenty-five years' service, has now been relieved by the power of enlistment for shorter periods."

All this is very true, and very good if it applied to the question of enlisting *young men*, or *fully-made men*; but the real point at issue is (when speaking of early recruiting) the question of enlisting *boys*, for, let them call themselves eighteen years of age, or what they will, the majority taken for the infantry are by far too young; and our author, in the passage just quoted, gives too unqualified a preference for such. Subsequently, when speaking of scrofula, he dwells forcibly upon its devastating effects in the army, and this, we feel certain, would not be the case to the same extent, were fewer of these young lads taken. Numbers do not constitute strength. A regiment with 700 well selected is more efficient than another with 750 doubtful men.

Were our author acting for himself, we do not believe he would fall into the same error of passing these boy recruits, for his remarks upon development, which follow, are both sound and good. It is as offering an opinion for the guidance of others who have not yet learned the amount of physical force requisite in the soldier to enable him to withstand the hardships of a military life, that we object.

We would prefer the tyro's being impressed with the following passage as that on which to ground his judgment as to the admission of a recruit:—

"The infantry soldier, from the nature of his employment, comprises a combination of characteristics which must always be recollected as necessary, and not merely desirable: he ought to be compact and strong." "The want of muscular power ought always, without modification, to be a sufficient warrant to cause his rejection."

This muscular power is not found in growing lads. It is a state that will, perchance, show itself, if unrestrained they are permitted to follow the plough for a couple of years, but will be prevented from development by a barrack life.

We must here close our notice of Mr. Massey's book, with the wish that its circulation may be general amongst the young medical officers of the army, as we feel assured they will derive information from its perusal.

Traité de Chimie Pathologique, appliquée à la Médecine pratique.

Par M. ALF. BECQUEREL et M. le DOCTEUR M.-A. RODIER.

Paris: Germer Baillière. 1854. 8vo. pp. 608.

THAT the study of chemical pathology has made very considerable progress even within the last few years, the valuable labours of Simon, L'Heritier, and Lehmann, on the Continent, as well as those of Bence Jones, Golding Bird, Prout, Garrod, and others in this country, afford abundant evidence; yet the field of investigation which the physiological and pathological conditions of the vital fluids lay open to the researches of those disposed to bestow a further amount of labour and industry thereon, appears to promise results of no inconsiderable value to the practical physician. About nine years have elapsed since MM. Becquerel and Rodier published a *brochure* "on the composition of the blood in health and disease," the principal substance of which is now reproduced in the second chapter of the work before us, which treats at very great length of the chemical relations of this important fluid.

The first or introductory chapter is short, and contains an account of the two fluids which concur in the formation of the blood,—namely, lymph and chyle. Of the former, only three analyses are given, and the authors conclude their observations on this part of the subject by candidly avowing that the physiological and pathological history of this fluid, so far at least as it rests on positive physical and chemical data, and not on hypotheses more or less absurd, remains to be made out. Of chyle there is but one analysis,—namely, that made by Dr. Rees, of fluid taken from the thoracic duct of a man recently executed.

Prefixed to the second chapter is an elaborate historical abstract of the various opinions regarding the importance of the blood which have been entertained from the remotest antiquity down to our own time. The first epoch, which embraces a long range of years, is rather the history of the hypotheses which successively swayed the minds of the learned than that of positive researches or accurate observations. These hypotheses, which have experienced almost as many variations as there have been different systems in medicine, have yet in their turn enjoyed great favour; in fact, they constituted the different systems of humorism. "Although long since fallen into just oblivion," say the authors, "we believe we ought not to pass them in complete silence, and we shall try to present a succinct *résumé* of them," &c. The second phase of the history of the blood's alteration comprehends the

earliest positive researches which have been made into the nature of this fluid. These researches are qualitative analyses, and they furnish numerous most interesting documents to physiological and pathological chemistry. The third and last phase comprehends the exposition of the quantitative analysis of the principal elements of the blood. It is only at the date of this epoch that the knowledge of the alterations which the blood undergoes has afforded any real assistance to physiology and pathology, and has cast any valuable light on questions of pathogenic import.

The process which our authors have adopted in their analysis of blood being of a complex nature, they have divided it into three series of operations: the object of the first is to furnish the density of the blood and that of the serum, the weight of the fibrine, globules, and solid matters of the serum collectively. The second series of operations should give the weight of extractive matters and of fatty matter. The third determines the weight of iron and that of the different inorganic, saline, or other principles which are contained in the blood.

The precautions necessary to be observed in each step of the analysis are detailed most minutely, but these, as well as the general description of the operations, do not admit of condensation. We may, however, notice some of the general results.

1. The affinity for water which the proximate principles of blood possess is very considerable, and this fact, in addition to their ready decomposition through an incautious exposure to too elevated a temperature, presents a considerable obstacle to the accurate determination of the albuminous matters contained in the blood; for, should the desiccation have been accomplished ever so effectually and carefully, the dried mass commences at once to reabsorb water from the atmosphere, and therefore should be weighed immediately while yet warm, &c. This deliquescent property the authors are disposed from their experiments to attribute to the presence of alkaline salts, uncombined soda, and soluble extractive matters.

2. When blood, freshly drawn, is not protected from atmospheric contact, a very appreciable quantity of water is vaporized, the amount depending principally on the extent of exposed surface, the temperature of the apartment, and the hygrometric state of the circumambient air. In hot summer weather, blood received into broad vessels becomes reduced by evaporation to one-third of its original volume. The time required to produce so considerable a diminution in bulk is

not mentioned. The authors recommend the use of deep and narrow vessels, and have given the results of three experiments made at a temperature of 61° to 62° F. in a moderately moist state of atmosphere. The mean of the three experiments represents the weight of the blood when first drawn = 16.867, and the loss by evaporation after twenty-four hours = 1.926. The containing vessel had a surface of 27 square centimetres. At a temperature of 50° F., two experiments were made, and the blood in the vessels had a superficial extent of 60 square centimetres. The mean result of these two is: weight of blood freshly drawn, = 47.875; loss by evaporation in twenty-four hours = 4.125. In order, therefore, to avoid this spontaneous evaporation, by which the more solid parts are concentrated, they recommend not only that the containing vessels be of the shape already mentioned, but that the air be hermetically excluded.

3. With respect to the density of the serum, this generally expresses the direct relation between the water and the solid matters therein contained; but this is not invariably the case, further experiments having proved that when there is an excess of extractive, saline and fatty matters collectively, compared with the amount of albumen, the density maintains a pretty constant ratio, but is always higher than when the albumen is in excess and the other matters are in comparatively small quantity. In some rare cases the presence of fatty matter alone in excess tends to lower the density still further.

4. When an individual is bled, provided this be not done rapidly, the different stages of the same bleeding furnish specimens of fluid which have different densities, the latter portions being less rich in solid matters than the earlier; but this difference is hardly perceptible till the bleeding shall have been carried to a certain extent; the impoverishment, however, is continuous and progressive. MM. Prevost and Dumas have long since made the same observation, and have explained the impoverishment of the blood in this way: "When we deprive a small animal of a notable quantity of blood, the veins rapidly absorb, at the expense of the rest of the system, an amount of liquid proportional and perhaps equivalent to that which has been lost to the circulating mass; whence it follows that the aggregate bulk of the particles appears to diminish in a given quantity of blood." In order, therefore, to represent by an analysis of the blood the condition of the animal from which it has been drawn, the first portion should be chosen.

The composition of the serum is sensibly affected by anterior bleedings; abstinence and the persistence of the disease

which necessitated the abstraction have also a considerable influence in rendering the blood less dense. The result of the alteration of the serum of the blood under the influence of two bleedings made at short intervals in twenty-seven individuals, who were all similarly affected, is given in a tabular form, to which, however, we must refer our readers.

The variations which the blood's composition undergoes in the various pathological states of the system are considered at great length and with considerable minuteness; the excess or deficiency of any constituent being established by a comparison with a model or standard of health previously deduced from a careful analysis of the blood of twenty-two individuals in health. There is some difficulty in constructing a formula which will represent in a general manner the composition of the blood in a healthy physiological state, due consideration being had for the thousand influences which tend to modify the results. According to Prevost and Dumas, who were the first to publish an analysis of the mean composition of the blood, this fluid contains in 1000 parts, 127 of globules, 3 of fibrine, 80 of solids of the serum, 790 of water. This formula, modified by M. Denis first, and afterwards by M. Lecanu, was nevertheless adopted by MM. Andral and Gavarret, and used by them as the *point de depart* to which they invariably referred in establishing general principles respecting alterations of the blood. According to the experiments of our authors, healthy blood contains 135 of globules, 2.5 fibrine, 80.35 solid matters of the serum, and 781.6 water, per 1000 parts.

In each great division of diseases the part played by the blood is made the subject of close investigation, and appended to the history of each is an exposition of the practical consequences to which the knowledge hence derived may conduce.

This chapter on the blood is so very long that to give even a *resumé* of the conclusions arrived at regarding the blood in the various individual diseases would occupy a considerable space; we shall therefore merely transcribe our author's remarks on typhus fever, and then pass on to the other parts of the work. This section commences by stating that, "in 1847 M. Henri Guenaud de Mussy and M. Rodier were commissioned by the French Government to enter upon an investigation of the typhus fever, which at this time was committing great ravages in Ireland. The former took charge of the pathological department, the latter was occupied with an analysis of the blood." After a polite acknowledgment of the reception they met with, and the facilities which were afforded them

in this country of pursuing their investigations both in the hospital and in the laboratory, they continue: "A complete work on this disease was to have been published, but circumstances have decided otherwise, and it is probable that this work will never see the light; one of us, however, has preserved the analyses of the blood of six patients affected with typhus fever, which we believe we ought to publish here in the hope that they may be of some use in the history of this disease.

ANALYSIS OF 1000 GRAMMES OF BLOOD.

	1. Man.	2. Man.	3. Man.	4. Man.	5. Woman.	6. Woman.
Density of the blood, .	1055.5	1047.0	1050.9	1042.1	1051.2	1041.2
Globules, ...	146.5	113.5	138.1	97.2	126.8	95.6
Solid parts of the serum,	75.4	71.1	60.1	72.0	70.0	71.7
Fibrine, ...	2.3	1.2	2.8	3.9	2.5	2.1
Water, ...	755.8	814.2	799.0	826.9	800.7	830.6

ANALYSIS OF 1000 GRAMMES OF SERUM.

	1. Man.	2. Man.	3. Man.	4. Man.	5. Woman.	6. Woman.
Density of the serum, .	1022.1	1021.1	1017.5	1020.0	1024.1	1020.0
Solid parts, ...	91.8	80.8	69.8	80.0	82.6	79.2
Water, ...	908.2	919.2	930.2	920.0	917.4	920.8

"It should be observed that these analyses were made previous to the period when the application of the polariscope to the study of albumen would have permitted a more exact appreciation of this latter element.

"The following conclusions may be drawn from these grave cases:—

"The *density of the blood* is, in general, diminished in a sensible degree.

"The *globules* in normal proportion in two of the men have been considerably below the average in two others; they have exhibited corresponding diminutions in one of the women.

"These facts being observed, we should examine into these deficiencies in the anterior state, the nature of the food, and particularly the anemic condition which pre-existed, the above modification of the globules not being altogether due to the disease itself.

"The *fibrine* was found either in normal proportions or

occupying limits inferior to the physiological state or in diminished quantity; this tendency to diminution, therefore, is a fact worthy of notice.

“The *density of the serum* is, in general, remarkably diminished; this diminution is, probably, the effect of the diet and the disease combined: it is, however, much more considerable in one case than in the others.”

The third chapter is devoted to an examination of the products of secretion furnished by the conglomerate glands. In this the saliva, bile, pancreatic fluid, the renal, spermatic, lacrimal, and lachrymal secretions, are successively examined.

In the fourth chapter the authors treat of the secretions of the mucous membranes, serous membranes, and skin. All the products of secretion from the surfaces are passed under careful review in their physiological as well as their pathological relations.

The fifth chapter exhibits the comparatively few results which science possesses with regard to the normal composition of the tissues and the pathological alterations which they are capable of undergoing. This chapter is rather an exposition of the *desiderata* of science in this respect than an account of the results which have accrued thereto.

The sixth and final chapter treats of the organic products of new formation, their physical relations, and chemical properties,—pus, tubercle, and cancer.

During the whole course of this work the authors appear not to have lost sight of the fact that investigations of this kind should possess a practical interest for the physician. The state of knowledge at present regarding the chemical constitution of the fluids, tissues, and organs, is represented in a manner sufficiently concise for purposes of reference, and at the same time with a degree of completeness that reflects the highest credit on the zeal and industry of MM. Becquerel and Rodier.

In fine, the volume before us is another of those highly valuable books for which we owe so much to the medical school of Paris; constituting a direct application of science to the practice of medicine, and thus converting the labours of the analytical chemist to their proper channel, so far as regards our profession and its objects. The low price at which it is published, and the facility which, from our intimate connexions with the French, their books may now be procured, fain make us hope that this, one of the most recent and best works on pathological chemistry, will obtain an extensive circulation in the British Islands.

Beiträge zur Lehre von den Knochenbrüchen. Von ALBRECHT THEODOR MIDDELDORF, Doctor der Medicin und Chirurgie; Praktischem Arzt; Wundarzt und Geburtshelfer; Docenten der Chirurgie an der Universität Breslau; Wundarzt am Hospital zu Allerheiligen; Mitglied Gelehrter Gesellschaften zu Breslau, Erlangen, Magdeburg, Paris.

Contributions to the Theory of Fractures of Bones. By ALBRECHT THEODORE MIDDELDORF, Doctor of Medicine and Surgery; Teacher of Surgery at the University of Breslau, &c. With five lithographed Plates. Breslau: Trewendt and Granier. 1853. 4to, pp. 150.

DR. MIDDELDORF'S work is divided into two parts—the first devoted to the consideration of fractures in general; the second, to that of individual or special fractures. The entire is derived principally from his own experience, based upon some hundreds of cases, of which he observed and accurately noted more than three hundred in the surgical section of the Hospital of All Saints, which was under the direction of the late Chief-Surgeon Alter, and, since 1850, under that of Professor Remer. The author states that “the unsurpassed works of an Astley Cooper, Boyer, Dupuytren, Malgaigne, R. W. Smith, &c., have stood by his side as faithful assistants in the laborious but certain path of seeking information at the patient's bedside, and have expounded and illustrated what he has himself witnessed.” The materials at his command were so abundant, that almost all, even the rarer fractures, have come under his observation.

The volume commences with a “statistical introduction,” from which we may cull one or two interesting results; thus it appears that in the city of Breslau, the population of which, at the time of the author's observations, amounted to 106,000, exclusive of military, 167 patients, suffering from fractures, were treated during the three years over which these observations extended, viz., in 1849, 54; in 1850, 65; and in 1851, 48; giving a yearly average of $55\frac{2}{3}$, and a monthly average of $4\frac{2}{3}$. The proportion of cases of fracture to the population was, therefore, in 1849, 1 to 1963; in 1850, 1 to 1630; and in 1851, 1 to 2208. During the same period, there were admitted into hospital, in 1849, 5829 patients from all causes (or excluding cases of cholera, 4852); in 1850, 4707; and in 1851, 4495 individuals; giving a proportion of cases of fracture to patients from all causes, in 1849, of 1 to 107.9 (or, excluding cholera, of 1 to 89.852); in 1850, of 1 to 72.4; in 1851, of 1 to 92.8. But if this investigation be confined to the surgical

division of the hospital, the result is, that in 1849 one patient out of every 26 surgical cases laboured under fracture; in 1850, the proportion was 1 to 24; and in 1851, 1 to 27; from which it would appear that, on an average, every twenty-fifth surgical case is one of fracture.

The author gives similar statistics showing the influence of age, sex, season of the year, rank, and occupation, and of the predisposition of individual bones of the skeleton in the production of fractures. As the influence of many of these depends, however, on particular circumstances, which must vary greatly in particular localities, we shall not enter into any particular account of them, but shall only remark in passing, that, of 179 fractures, 8 belonged to the bones of the head; 77 to those of the upper extremities; 38 to those of the trunk; and 56 to the lower extremities.

The author gives copious and well-arranged Tables, showing at a glance the details of the 167 cases of fracture already mentioned, under the heads (besides the columns of reference to the dates and books of the hospital) of name, occupation, age, duration of stay in hospital, cause, nature of the fracture, complications, treatment, result, duration of treatment, and observations.

Having disposed of the *etiology* of fractures, Dr. Middeldorf proceeds to consider the several varieties of these accidents. In speaking of them he remarks that, "besides the fissures which occurred in fourteen cases of fracture of the skull, not included in the Tables, I have hitherto observed only one instance of imperfect or partial fracture. This was an infraction of the middle of the right ulna. The patient, a boy of fourteen years of age, had met with a fall, and, though suffering pain, continued to work for eight days, when, in giving a violent blow with a very heavy smith's hammer, he suddenly experienced a cracking sensation, and became unable to work. On subsequent admission into hospital, the bone was bent towards the part of the forearm occupied by the extensors; the callus was still, however, capable of being easily placed in its proper position. We may here with great probability assume that an infraction had first occurred, which was converted into a fracture, partly by muscular action, and partly by the shock of the blow. Such accidents may indeed occur much more frequently in the ribs, but here they may very easily escape recognition, as we cannot as yet assume that a deep-seated pain and inflexion, following the application of violence, but without crepitation, and with inadmissibility of displacement, are with any certainty diagnostic of them. There are fractures

of the forearm, especially in children, which may easily mislead the unpractised observer to suppose that an infraction has taken place, in which we find the limb bent at an angle, capable of being raised without hanging loose, and manifesting a certain coherence as if there was merely a flaw in the bone, for we can perceive a motion which diminishes the angle; in most cases we very easily perceive a motion which increases the angle, but only with effort, and accompanied by a plainly audible rustling and commotion. Now if the latter motion be first made, it is supposed that a cohesion, such as exists in infraction, is present, but these are phenomena which are simply founded on the catching of the dentated fractured edges in one another."

We entirely agree with the author that the signs which he has mentioned are not, by any means, diagnostic of such a lesion as imperfect or partial fracture. We have always been of opinion (and we have examined recent specimens which confirmed our belief), that in these cases the bone retained its curved form, not in consequence of the osseous fibres upon the side of the concavity remaining unbroken, but from their becoming mutually impacted, and assuming a dentated or suture-like arrangement. A few years ago, Professor R. W. Smith laid before the Pathological Society of Dublin a specimen of a recent injury of the fibula, which had been supposed, during life, to have been an instance of incomplete fracture of its lower extremity; but an accurate examination of the preparation showed that, although the bone still retained its curved form (even after having been deprived of its periosteum by maceration), the fracture was complete, the osseous fibres, upon the side of the concavity of the curve, being mutually impacted so as to form an extremely firm suture.

The author believes that he has twice seen separation of the epiphysis, proceeding from direct causes, in the humerus of boys of fourteen and fifteen years of age. "The character of the crepitation, which was not that of rough, hard, spongy bones, but was a gliding one, as well as the other known signs, especially, in this case, the ease with which the sharp edge of the caput humeri could be felt, over which the vessels and nerves ran stretched like strings, and were thus compressed, producing a sensation of formication, the deviation of the direction of the axis of the shaft of the humerus, &c., made the diagnosis, which fortunately we had not an opportunity of confirming by dissection, very probable."

The following section is occupied with the symptomatology of fractures in general; the next with the consideration

of the diagnosis. The author concludes his review of the latter subject with some observations on the use of the acupuncture needle as a subcutaneous probe for the examination of a fracture. We shall not, however, at present follow him in his remarks upon this subject, as he proposes to investigate it more fully hereafter in a "Treatise on Examination with the Needle."

The remaining sections of the first part are devoted to the prognosis, the course and results, and the treatment of fractures in general. In the second or "Special Part" of his work the author treats of individual fractures, which he arranges under the four principal heads of fractures of the bones of the face, fractures of the bones of the trunk, fractures of the bones of the upper extremities, and fractures of the bones of the lower extremities.

The author describes with great minuteness the construction of a complicated double inclined plane for fractures of the thigh, capable of being employed also for keeping the limbs suspended, and designed especially for the use of public institutions; but as it would be difficult to follow his description without reference to the figures with which it is accompanied, we must on this subject refer our readers to the work itself. The lithographs which illustrate this and the other apparatus described in the course of the Treatise are admirably executed and highly explanatory, and we should be guilty of an omission did we fail to notice the excellent style in which the volume itself is brought out, being beautifully printed on most excellent paper, recommendations which do not always apply to continental works.

On Fatty Degeneration. By the late W. T. BARLOW, F.R.C.S.
London: John Churchill, 1853. 12mo, pp. 92.

THERE are few subjects of greater interest to the practical physician than that of which the volume before us professes to treat. The vital pathologist finds in the consideration of fatty degeneration ample material for reflection and study, since, being not unfrequently silent in its working and unsuspected in its progress, it may occur that the first notable indication of the change which has been effected, be evidenced either through its fatal termination, or under such circumstances as place at defiance the resources of medicine.

It cannot be denied that of late years extraordinary advances in our means of physical diagnosis have been perfected.

We are now, in microscopic examinations, enabled to confidently recognise those particular characteristics which indicate the very earliest stages of material change. The researches of the microscopist and chemist have, as it were, unfolded the whole map of morbid action, wherein we may trace the gradual progress of disease, and declare not only the nature of those changes already accomplished, but even premise those further alterations, which, pending the continuance of such action, would undoubtedly occur.

Having attained so much of certainty in our comprehension of life as evidenced by its effects on matter, let us inquire—Do we possess the same capability of inferring the nature of changes progressing in matter, from the concurrent vital manifestations? Or, in other words—Are we competent in the living structure, to pronounce the significance of each particular symptom with equal certainty as we can in the dead body declare the nature of any special appearance? Happily for mankind, observation of disease affords a negative reply, since the whole category of latent affections, as well as the progress of many visible alterations, declare, physical changes may progress without producing appreciable derangement of function until such time as the structures they involve prove inadequate to the purposes of the economy. Conditions are detected after death which had never been suspected during life, and symptoms during life exist inexplicable by post-mortem examination. The human organism is thus proved to possess a power of vital accommodation, by which the action of its structures is maintained though the elements entering into their constitution be materially disarranged. Were it otherwise, existence would be dependent on endless contingencies, since disease and death should be identified with each other.

We admit that this latency of many diseases is a principal source of their fatality. If we be asked—Is the fact that morbid changes may attain a most formidable extent previous to their discovery, to be regarded as a proof of the inaccuracy or incompetency of medicine? we reply, no more than our incapability of explaining many visible phenomena argues the incorrectness or insufficiency of the natural sciences. In the contemplation of nature there are many sensible operations which we must be content to study through their effects, even though in our observation of disease those effects imply the destruction of powers whose perfection constitutes the duty of the physician. Such a question involves not only our just estimation of the relation which the material structures hold to each other, but also our knowledge of how far these separate structures

contribute to the essential purposes of our being, conjoined with our appreciation of that general pervading influence which insensibly guides and directs the whole, each tending to prove that it is not in the narrowness of science but the vastness of life—not in the limited character of material changes, but the unlimited and variable nature of immaterial influences—the answer to such an inquiry rests.

We have already premised that microscopy and animal chemistry have enabled us to measure with extraordinary precision the results of aberrant vital force, and in advancing our knowledge respecting those transmutations of organic elements which occur in the living body, have thus imparted to us in many affections increased confidence in our prognosis and treatment; yet, inasmuch as the nature of this change we are considering does not admit of such demonstration except through the analysis of its ultimate effects, whose presence may remain either undetected or beyond our reach, until the extent of its development argues a confirmed condition of the system; we are the more fully impressed with the necessity of closely studying its vital as well as cadaveric pathology, since it is in our estimation of their mutual relations we can alone hope to solve the difficult, we might almost add, the anomalous problems, affections of this character not unfrequently present.

These opinions have been strengthened by our study of Dr. Barlow's work, which, replete with the views of many of the best and most recent authorities, has afforded us both profit and pleasure in its perusal.

Repudiating the supposition that microscopical appearances are in themselves sufficient to explain the nature of many changes in their immeasurable relations to decay and death, Dr. Barlow proceeds to direct particular attention to the association of muscular atrophy with muscular degeneration, and impresses the true signification of the former term, which he regards as implying not alone an obvious wasting of the affected tissue, but also a deficiency of its perfect nourishment. On the latter view it is quite explicable that real atrophy and seeming hypertrophy of a part may coexist, as is exemplified by hearts which are at once fatty and enlarged, showing increased bulk without corresponding power. As the causes of atrophy and degeneration are believed by the author to be identical, both blending inextricably and forming very often descending steps of one downward course, and as it seems impossible to conceive how any part can ever be transformed into a lower material without first suffering an impairment of nutrition, Dr. Barlow suggests that in the due consideration of the latter the

rational study of the former rests, and accordingly proposes for our inquiry some of the principal causes which conduce to such an end, those causes admitting of being ranged under one of the following heads:—1st. A wrong or defective state and composition of the blood. 2nd. An insufficient supply of blood. 3rd. A deranged or obstructed influence of the nervous system. 4th. An imperfect, unhealthy, or declining state of the parts to be nourished. These conditions, be it observed, being in every respect the reverse of those which Mr. Paget has specified as most important in order to the completion of perfect nutrition.

In corroboration of the first proposition, the assertion of Harvey, that the blood is “the cause of youth and old age,”—the observations of Simon respecting the relations of the blood corpuscles to the process of nutrition in different periods of life,—the researches of Mr. Canton, establishing the true nature of the arcus senilis,—are considered as together confirming the author’s views;—that closely associated with changes in this fluid is that material perfection and vital energy, whose declension and decay has been so ably described by Sir H. Halford as being symptomatic of the “climacteric disease.” Notwithstanding Sir Henry’s opinion that “a disease rather than a mere declension of strength, and decay of natural powers,” is indicated by this “climacteric” condition, Dr. Barlow still believes that it may be in many instances attributed to the form of degeneration we are considering, to which, under such circumstances, he observes: “we can hardly affix the term abnormal or morbid.” Dr. Marshall Hall’s views on “the decline of the vital powers in old age,” and the symptoms he sets forward as indicating the same, when compared with those of the fatty heart, as stated by Dr. Quain, appear so perfectly analogous, that Dr. Barlow is led to believe in their close identification with each other, while the failure of function which degeneration produces, when contrasted with those temporary disturbances consequent on chlorosis and other affections of the blood, are regarded as tending to further establish the great practical inference that this abnormal condition, and the natural decay of an organ, though admitting of widely different explanations for their immediate predisposing causes, may in their essential nature be considered as identical, both indicating beyond question, impairment of nutrition.

The ravages of disease, and the progress of time, may in their operations anticipate each other. We find this special form of degeneration is not confined to any particular age, but is to be met with, either supervening on other affections, and

thus in them originating the most dangerous complications, or itself laying the foundation of affections, usually, we might almost add, necessarily, of a fatal character.

The observations of M. Louis, M. Bizot, and Dr. Ormerod, respecting the association of the fatty heart with phthisis,—of M. Louis, Dr. Latham, and Dr. Stokes, upon the relation of the softened heart to continued fever,—while establishing the truth of the foregoing remarks, are considered as further illustrative of the author's views:—that defective nutrition may lead to degeneration as well as softening of the tissues. Having noticed that such a complication in phthisis may offer one explanation of those cases of sudden death which sometimes occur in that disease, we read the following:—

“ But how are we to tell whether this change of structure of the heart, supposing it to be detected after death, has preceded the fever, or occurred during its course?”

In reply, we may be permitted to refer our readers to the descriptions of both Louis and Stokes, who have with faithfulness and perspicuity detailed what we may term the specific appearance of the fever heart, the latter of whom, in his original researches on this important subject, published in the pages of this Journal many years since, particularized such conditions as, to the attentive observer, could scarcely fail to be sufficiently diagnostic of the post-mortem appearance in question, while, at the same time, he laid down such directions for its recognition during life, as render it scarcely possible its presence, if suspected, could remain undiscovered.

The occurrence of typhus fever in a patient whose heart was, previous to its accession, in a debilitated condition, presents one of the most formidable combinations of disease with which we are acquainted; the importance of whose recognition, we believe, cannot be too highly estimated, since in treatment it affords additional confidence in the early and free use of stimulants, while in prognosis it imparts greater caution, as from the coexistence of two causes, each adequate to place the vital powers in abeyance, [a protracted and dangerous illness, or at the best, a tedious convalescence, may be anticipated. We cannot lay down any physical signs which might be considered as characteristic of this compound condition, excepting those already given by Dr. Stokes as indicative of typhoid weakness, whose earlier manifestation and more decided development, when compared with the previous history and concurrent condition, might lead us to infer such a special state.

The fact fully and clearly illustrated by Dr. Quain, that

obstruction of the coronary artery may lead to local fatty degeneration of the heart—the connexion between softening of the brain and narrowing of the arteries supplying the affected part—the atrophy and decay which, generally speaking, follow arrestations or diminutions of the current of blood, as observed by Mr. Simon to occur in the kidney, consequent on the obstruction of a small artery, are considered as sufficient proofs:—that an inadequate supply of appropriate blood must be also regarded as one of the principal causes of this change. However, we may, from special circumstances, be enabled to speak of the effects of such a cause on any particular structure, it is difficult to form an estimate of the general influence of this uncomplicated condition, since the questions of simple deficiency and morbid changes of the blood are to a great extent inseparable:—the association of anemia with spanemia, or the coexistence of deficiency and depravity of blood, being fully confirmed by Mr. Simon's analysis of that fluid in melæna, and M. Andral's investigations in anemia, the one showing the deficiency of fibrine, the other, the diminution in the proportion of the blood corpuscles,—which conditions, according to Dr. Ormerod, were associated with the most marked degeneration of the heart.

That the occlusion of the minute vessels of a part may be the consequence and not the cause of the locally developed affection, Dr. Barlow considers the condition of the pulmonary vessels in an inflamed spot of lung, as noted by Mr. Paget, as well as the coagula in the vessels of a mortified limb, fully establishes. To uniformly distinguish that occlusion which precedes, arises in the course of, or is superadded to any local affection, presumes a certainty in data we at present do not possess, nor could we, were we enabled to do so, isolate such conditions as the cause of this disease, since, to use the words of the author, "it is often quite impossible to say how much is due to the state of the vessels, how much to the condition of the blood itself, and how much to the defective assimilation of tissues, the latter of which has often most undoubtedly a large share in the process of destructive conversion."

The occurrence of sanguineous apoplexy in a state of anemia is believed to depend on fatty degeneration of the cerebral vessels, a subject on which the researches of Mr. Paget have thrown much light, while the writings of Dr. George Burrows and Budd further support the doctrine, that all chronic and debilitating maladies may so impair the nutrition of the cerebral blood-vessels as to lead to their degeneration and rupture, thus giving rise to an analogous condition to that described by Dr.

Quain, where obstruction of the coronary artery eventuated in local fatty degeneration, under which circumstances apoplexy may occur in persons having, to use the words of Dr. Burrows, "the very reverse of the apoplectic make."

In his observations on the influence of the nervous system, and its relation to the progress of nutrition, much that is interesting is to be found. That action and reaction of mind and body, which many authors have previously noticed, is by Dr. Barlow particularly dwelt on. The influence of the emotions on the secretions as detailed by Dr. Carpenter—the effects of mental attention on the bodily organs as set forth by Dr. Holland—the researches of Lallemand and Rostan on the influence of the depressing passions—the fatal consequences observed to follow long-continued anxiety and undue mental exercise as described by Dr. Forbes Winslow, illustrated by the pallid cheek, the attenuated frame, the careworn brow and early grave of those many whom genius fostered that fortune might crush,—demonstrate too sadly the close association and mutual dependency of the material and immaterial constitution, as well as their ready co-operation for the production of disease.

That there is a local failure of the assimilative power which is always the immediate and necessary precursor of degeneration, the circumstances under which it is developed appear to indicate. The interesting point in such an inquiry to be determined is, whether a part be unable to assimilate from want of material, or power to appropriate it. To solve this question we must first be in a position to trace the steps in the morbid process, and to determine, not only that eclectic power whereby parts abstract, but also that special power by virtue whereof they perpetually renovate themselves; and, while making due allowance for the result of combined agencies, to also estimate the relative importance of those separate operations whose aggregation constitutes this influence. Such knowledge, in its relation to the disease we are considering, we can only hope to more closely approximate by our just comprehension of the laws of nutrition, since therein rests our capability of estimating the germ power, and the influence it exercises through all our being. In the progress of life is demonstrated the process of decay. Our capability of recognising the one will be therefore proportionate to our intimate knowledge of the other.

The researches of many microscopical pathologists have established the character of those several changes in the organic cell which precede degeneration, and noted the successive stages of germination, growth, and death. That their labours have not eventuated in corresponding accuracy in treatment, has by

many sceptics been advanced in depreciation of their practical advantage. Had M. Andral accomplished no more for science than the establishing the compound nature of many pathological phenomena which the casual observer might consider as simple, his labours had not been in vain; for therein rests the refutation of those doctrines which limit or estimate the varied phenomena observable in living beings, according to the operation of ordinary physical or vital forces, while at the same time, the cause of the uncertainty in treatment is thereby in a great measure explained. Since, as Dr. Williams has already shown, to correct those primary changes in the organic elements which seem to set at defiance the ordinary laws of nutrition, and even-tuate in such transmutations as those under consideration, would imply our capability of not only commanding uniformity in vital action, but also of accurately measuring vital force;—a supposition, which, though so thoroughly erroneous, is still, we fear, entertained by some who regard natural and vital laws as identical, and presume that the same sensible operations observable in the interchange of organic elements, when removed from vital influences, must be accomplished in a like manner, follow a similar course, and be equally as appreciable when governed and directed by such influence; forgetting that the very existence of the conditions so specially examined is the evidence of the accomplishment of a certain change, which may be either the simple result of many primary operations, or the compound result of many secondary operations, of whose individual nature we are unable to speak, except through the complicated evidence their consummation affords.

Mr. Paget has well described the primary development of the cell, the deterioration of its nucleus, and consequent blighting of its germ, as observed by him to occur in fatty transmutation of muscular structure; such transmutation, according to that distinguished writer's views, must therefore be regarded as so far bespeaking irreparable decay, since the instruments of reparation, *quoad* the structures involved are wholly destroyed. From this affirmation the very important inquiry arises, wherein then rests our hope for the patient? To this Dr. Barlow replies, "not in the future changes of the part destroyed, but in the efforts of the bordering tissue—not in absolute death, but reparable atrophy." We admit experience too frequently verifies Mr. Paget's opinion that "a tissue once converted into fat may be held to have perished irrecoverably." Are we, however, to give up in despair, and to presume that uniformly this abnormal condition is beyond the reach of the general surrounding vital influence, because it is incapable of

manifesting special vital force? Or, should we, with Dr. Stokes, rather entertain the hope, "that with the advance of medicine, both diagnostic and hygienic, we may yet be enabled not only to check the growth of fat in the heart, but to restore the muscular fibre to its pristine condition of volume and power?"^a That Dr. Barlow believes much good, be it curative or prophylactic, may be expected from the employment of suitable means, we infer from his advocating, according to the circumstances of the case, repose; the administration of iron; such change of air and scene as entails cheerful occupation for the mind and healthful exercise for the body, which he declares he has found, not only to arrest the atrophy which would have become degeneration, but also to stay the latter where it had begun.

We have so far, analytically rather than critically, reviewed the first part of "*Barlow on Fatty Degeneration*." We cannot at present enter more fully into the subject; we trust, however, on a future occasion to do so, being fully impressed with the practical importance of those researches, to which, as his writings testify, Dr. Barlow must have devoted much time and great labour. Yet, ere concluding these observations, we shall briefly notice some contributions, which, emanating from this side of the Channel, have considerably advanced our knowledge on this subject. We are the more particularly induced to do so, since, on the present occasion—as on others—those contributions have been to a great measure ignored.

In his recent work, alike remarkable for the vastness of its research as the philosophy of its reasoning, Dr. Stokes has fully, and we believe impartially, set forth those sources from which the most valuable additions to our knowledge on this subject, in connexion with the heart and great vessels, have been derived.

The character of the apoplectic seizures, which may be considered as indicative of fatty degeneration of the heart, was many years since described by Dr. Cheyne^b, and subsequently confirmed by Mr. Adams^c, the latter of whom, in his *Pathological Descriptions*, also established the association between atheromatous and fatty depositions, which, although the researches of Andral, Lobstein, and Gluge, have not tended to the same result, more recent observations approve. The demonstration, by Professor Smith^d, of free oil in the blood, while showing that this fluid is sensibly affected in the change,

^a Stokes, *Diseases of the Heart and Aorta*, p. 356.

^b *Dublin Hospital Reports*, vol. ii. p. 217.

^c *Dublin Hospital Reports*, vol. iv.

^d First Series of our *Journal*, vol. ix.

leads Dr. Stokes to the further inference, that "a fatty state of the heart may be caused, not alone by degeneration of the protein compounds, but also by oil already formed, and circulating in the blood itself"^a. The cases detailed by Dr. Townsend, expounding the latency of this disease^b—the accurate detail of the physical manifestations, denoting progressive and matured softening of the heart, as witnessed in fever—the association of its vital and mechanical operations as a means of diagnosis in fatty degeneration, which Dr. Stokes has recorded both in the pages of this Journal, and in his great work, to which we have alluded—the association of certain conditions of the heart, with the occurrence of delirium, as a guide to treatment in fever, as set forth by Dr. Hudson^c—the eminently practical contributions to our knowledge of disease of the brain as dependent on disease of the heart, from the pen of Dr. Law^d—the more recent essay of Mr. Richardson on "Permanently Slow Pulse and Fatty Heart"^e,—and the numerous contributions to our knowledge of the vital and physical evidences of this disease which are to be found in the writings of many throughout our pages, conjoined with the records of the "Dublin Pathological Society,"—sufficiently prove that hitherto we have been neither negligent nor careless observers, since much of what is anatomically true, and more of what is practically useful in our recognition and treatment of this disease, has been noticed by and is fairly due to the labourers of the Irish Medical School, thereby affording the best reply to the energetic declamation of certain alarmists, who, in their vivid expositions of our small faults, seem to have manifested an equal indifference to our great merits.

Finding by the title-page that this was a posthumous publication, we were induced to refer to that highly useful work, "The Medical Directory," for the present year, in the obituary of which we read as follows:—

"His—Dr. Barlow's—illness, had been preceded by some circumstances of an irritating and vexatious character, which had kept his nervous system in a state of much excitement; some regulations, emanating from the weekly Committee of the hospital, requiring his signature each time he visited the wards, together with other changes in his department, harassed and annoyed him; headach of some days' duration, with sleep-

^a Stokes, *Diseases of the Heart and Aorta*, p. 320.

^b First Series of our Journal, vol. i.

^c First Series of our Journal, vol. xx.

^d First Series of our Journal, vol. xvii.

^e No. 28 of our present Series.

lessness, were followed by febrile symptoms and exhaustion, which terminated fatally."

A statement of this nature is always very painful to us, for the question arises to our mind, why can such things be? We have had some opportunity of forming an opinion, and recalling our own experience as well as our observation of others; and our conviction becomes the more confirmed, that the possibility of similar occurrences rests not in "our stars, but in ourselves." Do we, as a body, uphold and support each other? Is our profession recognised with that due honour, and treated with that proper respect, which the vastness of the interests it involves, and the responsibility of the duties it entails, justly entitle it to? Is its social and scientific position identified? Experience, we regret to say, affords a negative reply. Whence are we to hope for redress? Not surely from public boards, who value our services at a lower rate than those of ordinary mechanics; not surely from corporate bodies, who, in conflicting interests, seem to lose sight of those great principles committed to their care; nor yet from despotic Committees, in whose hands it appears not only the duties of our office, but its very existence, rests. But rather, in that reciprocal confidence and mutual co-operation one with another, which, while affirming our just claims as a profession, at the same time will lend stability to our position as individuals; and this can only be effected by honourably upholding each other, rather than following in that course which we have already strongly repudiated, of misleading the public respecting our claims to its good faith, and so affording fictitious grounds for their unjust suspicions or oppressive enactments.

A Treatise on the Diseases, Injuries, and Malformations of the Rectum and Anus. By T. J. ASHTON, Surgeon to the Blenheim Dispensary, formerly House Surgeon to University College Hospital, &c. London: Churchill. 1854. 8vo, pp. 356.

IN the twelfth volume of our former Series (1838) will be found a review of a *Treatise on Diseases of the Rectum* by the late Dr. George Bushe, a surgeon of distinguished abilities, who received his education in this city, and who left this country to fill the Chair of Anatomy at New York. In Mr. Ashton's preface we find the following testimony to the value of Dr. Bushe's work:

"The treatise of Dr. Bushe, of New York, who unfortunately died shortly after its publication, is the only one with which I am acquainted that embraces the whole subject; and from it I have gained much valuable information."

This paragraph, and an occasional mention of Dr. Bushe's name, constitute the only acknowledgment Mr. Ashton sees fit to make for copying, with singular fidelity, a great part of Dr. Bushe's book. We shall lay before our readers a few proofs of this wholesale, and we are happy to say nearly unexampled act of literary piracy in the present age. Were we to bring forward in evidence all the parallel passages in the two volumes we should be obliged to transcribe a large portion of each work. We must, to expose so flagrant a dishonesty, contrast some passages, and we shall merely premise that the following selections are taken almost at random:

BUSHE, page 93.

*Inflammation and Excoriation of
the Anus.*

"These affections are generally combined, and may be produced by long-continued walking, or riding on horseback, the passage of irritating secretions, or the want of cleanliness. Obesity and warm weather strongly predispose to them. When they arise from excessive walking, or riding on horseback, nothing more will be necessary than to keep the bowels easy with enemata, to wash the parts three times a day, to dust them with hair-powder, or lapis calaminaris, to place a fold of old linen between the buttocks, and to enjoin the horizontal position. Should they depend on irritating secretions, produced by cathartic medicines, they will subside when the purgation ceases. If they coexist with diarrhœa or dysentery, they will disappear with the cure of these diseases. When the secretions, however, become vitiated from luxurious living, it will be necessary to enforce a vegetable

ASHTON, page 14.

*Inflammation and Excoriation of
the Anus.*

"If the inflammation and excoriation are the result of excessive exercise either on foot, horseback, or riding many hours in a carriage, it will only be necessary to wash the parts two or three times a day, and to apply pounded lapis calaminaris, or hair-powder, and to keep a fold of lint or linen between the buttocks; it may be sometimes advisable to enforce the observance of the horizontal position. Enemata will be the best means of keeping the bowels open. Should the cause depend on the depraved state of the excretions, this condition must be remedied by the exhibition of appropriate medicines; small doses of mercury and chalk, with extract of taraxacum, or blue pill with hyoscyamus and cathartic extract, to be taken at night, and the following morning, Rochelle salts with infusion of senna, or a bitter tonic infusion; the sulphate of magnesia, dilute sulphuric acid, and the compound infusion of

diet, to exhibit blue pill and cathartic extract at night, and Rochelle or Epsom salts, in an infusion of senna, quassia, or some such preparation, on the following morning. This course should be continued until the alvine discharges become healthy. The local treatment necessary in each of these cases is similar to that specified above: when they arise from want of cleanliness, the hair and discharge become matted together, and thus form a crust which covers the excoriated surface. Under such circumstances, the parts ought to be poulticed until the crust becomes so soft that it can be removed without cutting the hair; for should this be done, as I have once seen, the irritation created by the stumps will increase the inflammation, protract the healing of the suppurating surface, and render the patient exceedingly uncomfortable, until the hair has again acquired sufficient length to diminish the friction of the buttocks on each other. After the parts are sufficiently cleansed, a saturnine cataplasm impregnated with laudanum should be applied, and changed every six hours, at which time the diseased surface ought to be washed with cold water and the common yellow soap. An emollient lavement may be taken daily, the horizontal position maintained, and a low diet strictly observed. In some cases, particularly those of long standing, it is sometimes necessary to use lotions of the sulphate of zinc, or nitrate of silver. The ointments of the oxide of zinc, superacetate of lead, white precipitate or nitrate of mercury, are also very useful remedies."

gentian, or infusion of cascarrilla, make a good purgative. Other similar combinations may be prescribed; the remedies are to be continued until the alvine discharges become healthy. The same local treatment as that previously recommended must be adopted; if dysentery or diarrhoea be the cause, the effect will subside with the cure of these diseases; if the abuse of cathartic medicines has set up the disease, by the discontinuance of the cause the effect will be removed. When inflammation and excoriation have been produced by a neglect of cleanliness, the observance of different habits is the first step towards a cure; soap and water must be used several times daily; if the hair around the anus has become matted together by the discharge and filth, forming an incrustation over the excoriated surface, it must be softened by the application of linseed-meal poultices, and the free use of the hip-bath and soap; on no account must it be removed by cutting the hairs, otherwise the stumps left will cause much irritation and distress, until they have again attained a certain length.

When the parts are sufficiently cleansed, poultices impregnated with opium and a solution of acetate of lead, or lint saturated with lotions of nitrate of silver, sulphate of zinc, or acetate of lead, may be kept to the parts; or ointments of the nitrate of mercury, bichloride of mercury, oxide of zinc, &c., may be applied. The recumbent position must be maintained, and the bowels acted on by cooling laxatives and emollient enemata."

Again, in the chapter on fissure of the anus and lower part of the rectum, we find the following:—

BUSHE, page 102.

“In the commencement of this disease the symptoms are not severe, being merely at one time a pricking or stinging sensation, at another a slight smarting in a certain point of the anus, which, under the use of lavements and low diet, subside either altogether, or, after a few days, return with more severity. The pain, gradually increasing, becomes burning, sometimes lancinating, and, when severe, throbbing. It is increased by forced expirations, as coughing, sneezing, and urinating. Every effort to discharge gas and fæces is attended with excruciating torment, which continues for one or more hours, attended with violent spasmodic contraction of the sphincter ani. So violent is the agony, that most persons thus afflicted put off the calls of nature, maintain the recumbent position, and some even avoid taking a proper quantity of nourishment for fear of increasing the fecal mass. The pain is always increased by stimulating food, and in females during menstruation. . . . When the fæces are solid they are slightly streaked with blood and matter, and when more soft, are figured, and of small size.”

In the above quotations which we have made from Mr. Ashton no reference is made to Dr. Bushe's work; in the next extract which we shall give there is an allusion to it, but with what sort of candour or honesty our readers may judge for themselves.

BUSHE, pages 152-54.

“I have frequently dissected them (internal piles) with the greatest care, and found that

ASHTON, page 32.

“In the commencement of this disease the symptoms are not generally severe, and are only experienced when at stool, when, at a certain point, there will be smarting of greater or less severity, or perhaps only a slight stinging or pricking sensation may be felt; if the disease is allowed to progress, the smarting, during the act of defecation, will be greatly increased, or the pain may be burning or lancinating, followed by excruciating aching and throbbing, with violent spasmodic contraction of the sphincter muscle, continuing from half an hour to several hours. The stools, when solid, will be streaked with purulent discharge, and slightly with blood, and when more soft will be figured, and of small size. . . . The disease being fully established, the pain will be induced by sneezing, coughing, forced respiration, and by micturation, and so violent does the agony become, that individuals thus afflicted even avoid taking the proper quantity of nourishment, in order that the fecal mass may be small; they also in their dread postpone the calls of nature . . . or may be compelled to remain recumbent.

ASHTON, pages 84-86.

“By dissection internal, hemorrhoidal tumours will be found to consist of both arteries

they were spongy, reddish, and contained both arteries and veins, the latter being most capacious, but always perfectly healthy. Their surface is villous, and generally bleeds when touched roughly, or scratched with the nail, the blood which issues being of a florid colour. In many instances I have been able to rub off exceedingly vascular and fragile adventitious membranes from their surface. Thus it would seem that they may acquire an increase of magnitude in this way."

and veins, the latter capacious, not in a diseased condition, but merely of abnormal development; the areolar tissue of the mucous membrane is hypertrophied. . . . The surface of the tumours is villous, presenting to the unassisted eye a granular appearance; they generally bleed freely if rudely touched, or accidentally scratched by the nail during an examination, the blood being of a bright red colour. Dr. Bushe states he has been able to rub off an exceedingly vascular and fragile adventitious membrane from their surface, and is of opinion that they may thus acquire an increase in magnitude."

We shall not weary the patience of our readers by making any further extracts at length; in order, however, to prove that these are no accidental or occasional instances of identity of opinion and expression, we shall add references to some others, which are as flagrant.

Remarks upon the symptoms of hemorrhoids, in pages 86, 89, 90, 91, of Ashton, are taken from Dr. Bushe's book, pages 154-55, 146-49, 148, 149, 146, 147. Pages 91 and 94 are a literal transcript into the text from a long note, beginning at page 147 of Bushe. The chapter on foreign bodies in the rectum opens with paragraphs taken from the 57th page of Bushe, and the bulk of the cases recorded in it is taken from the notes to his chapter on the same subject. The chapter on malformations of the rectum and anus is also made up by similar wholesale plagiarism, and this, with a degree of care not to be anticipated from the barefaced way in which the extracts given above have been copied. In this chapter there is less addition of verbiage (we cannot call it original matter), so that the English author resorts to transposition of paragraphs from the American, e.g. :—

	BUSHE.		ASHTON.
Page	36, 38,	Page	326.
"	44, 38, 39.	"	327.
"	49.	"	328.
"	46, 51, 52, 53.	"	330-31-32.
"	55, 54, 41, 50.	"	334-35-36-37.
"	43.		First part of page 338.

It was not enough, however, for Mr. Ashton to rewrite Dr. Bushe's work, and publish it as the offspring of his own brain (with the addition, to be sure, of certain facts and opinions which have gained general acceptance since the year 1837). His sins of bookmaking do not end here:—the book, such as it is, consists of 356 pages; upwards of 80 of these are filled with cases, many of them, as already stated, taken from Dr. Bushe without acknowledgment. One page is taken up with Montégre's classification of piles, which, we are told in the next paragraph, is unnecessary for practical purposes. Prescriptions are introduced into the text in such a way as to occupy much space^a; the table of contents is expanded to twelve pages, at least eight of which might have been dispensed with, as a copious index of six pages supplies the information contained in them. We shall not complain of the large type and ample margin, nor of the numerous subdivisions into chapters and paragraphs; these are common sins, and, to a certain extent, venial; the eyes of the reader are saved, and there is abundant room given him for reflection: yet if all writers of monographs were to expand their works in this direction as much as Mr. Ashton has done, our libraries would soon be enlarged to a portentous size.

In conclusion, we shall only suggest to Mr. Ashton, when he again ventures upon wholesale plagiarism, to select an older and less remarkable book than that of Dr. Bushe; and above all, to shun the works of our countrymen, especially of those whose talents have raised them to conspicuous positions in foreign lands, for our feelings of national pride are certain to make us well acquainted with their contents, and we shall surely not spare those who rob their ashes.

Sleep and Dreaming. A Lecture delivered before the Cork Young Men's Association, during its third session. By JOHN POPHAM, A.B., M.D.

THE subject selected by Dr. Popham for his lecture before the Young Men's Association of Cork, being one which has alike

^a Among these we find Dr. Graves' excellent recipe for habitual constipation (of course unacknowledged):—

GRAVES.	ASHTON.
R Electuarii Sennæ, . . . ℥ij. Bitartratis Potassæ, . . . ℥ss. Carbonatis Ferri, . . . ℥ij. Syrupi Zingiberis, quantum suffi- cit ut fiat electuarium.	R Confectionis Sennæ, . . . ℥ij. Potassæ Bitartratis, . . . ℥vj. Ferri Carbonatis, . . . ℥ij. Syrupi Zingiberis, q. s. M. Fiat Electuarium.

attracted the attention of physicians, philosophers, and poets, was therefore peculiarly adapted to such an audience.

Bringing to bear on its consideration a classic mind richly stored with varied knowledge, Dr. Popham ably accomplished that in which so many fail; and happily conjoining the solid truths of science with the vivid suggestions of fancy, embodied at the same time, in his eloquent address, many profound theories and deeply interesting facts.

Dr. Popham's pamphlet must be read with improvement and pleasure by all desirous of gaining information respecting those topics of which it so well treats, while the high ethical tone evident throughout its pages cannot fail to at once impress the reader with the unaffected zeal of the author, as well as the great practical advantages which must accrue to such Associations from the delivery of similar addresses by individuals so eminently competent and trustworthy.

Traité Clinique et Pratique des Maladies des Vieillards. Par M. DURAND-FARDEL, Docteur en Médecine de la Faculté de Paris, &c. Paris: Germer Baillière. 1854. 8vo, pp. 876.

It is not a little singular, considering how medical literature abounds in treatises on the diseases of infancy and childhood, the small number of books which has been published on the peculiarities that characterize the diseases of old persons, or the special maladies to which they are subject. We made a somewhat similar observation some years ago, when reviewing the treatise of Dr. Day in the pages of this Journal, and since the publication of his volume, in 1849, we are not aware that any special work, except that of the late Reveillé-Parise^a, has appeared on the subject. That this does not arise from any want of interest in the inquiry, or material to write upon, is sufficiently proved, so far as regards our French brethren, by a reference to their periodicals and medical dictionaries, for in them are to be found, first, a valuable memoir "On the Diseases of Old Age," by M. Prus^b; an admirable, concise essay, by M. Beau, entitled: "Clinical Studies on the Diseases of the Aged"^c, and an excellent article on the "Diseases of Advanced Life," by M. Gillette^d. Yet no English medical writer since

^a *Traité de la Vieillesse, Hygiénique, Médical et Philosophique.* Paris: 1853.

^b *Mémoires de l'Académie Royale de Médecine*, 1840.

^c *Journal de Médecine*, 1843.

^d *Supplément au Dictionnaire des Dictionnaires de Médecine*, 1851.

Dr. Day has thought it worth while to devote a special treatise to this subject, and therefore his work, though deficient in many respects, and devoid of much originality, is still the only standard book in the English language to which the practitioner seeking for information on the peculiar features which characterize the maladies of advanced life can refer.

M. Durand-Fardel, then, has supplied a deficiency much felt in medical literature, by laying before the profession the results of a prolonged study of the maladies of the aged during the many years he has been attached as *interne* to the asylums of Bicêtre and Salpêtrière, and of a special attention to these diseases for fifteen years. More than ten years ago he published a highly practical treatise on softening of the brain, in which he dwelt especially on this affection in the old; and stimulated by the reception which it received, he says that he has been encouraged in his present enterprise, and emboldened to submit to the judgment of the profession this extended work.

The volume commences with an introduction, containing an admirable exposition of the several points in anatomy, physiology, pathology, therapeutics, and hygiene, which are special to old age, and a correct knowledge of which is necessary to the understanding of the deviations from a healthy state that may occur in advanced periods of life.

The work itself is divided into four parts; in the first of which diseases of the brain and nervous system are treated of; in the second, diseases of the respiratory organs; in the third, diseases of the circulatory system; and in the fourth, diseases of the abdomen. To these is added an appendix containing two chapters, on gout and on diseases of the skin.

The diseases of the encephalon are, perhaps, those deserving most attention in a book devoted especially to an account of the maladies of old age, as they are most frequent at this period of life, and can then be most successfully investigated; the author has consequently applied himself most carefully to their examination, incorporating in his present observations what he had previously published twenty years since in his valuable work on *Ramollissement of the Brain*, and thus we are here presented with the most perfect essay on this important class of affections which has been ever written.

Their special description is preceded by a short inquiry into the difference between the diseases of the brain in infancy and childhood, and in advanced age. The most remarkable contrast existing between them being, that those of early life are affections of the membranes or periphery of the nervous centres, while in old age it is the hemispheres or central parts which

are affected ; the diseases most frequently occurring at the latter period being cerebral hemorrhage and ramollissement. The several affections of the brain are then considered as follows: 1. *Cerebral Congestion*, which is most fully inquired into, both as regards its pathology, nature, and seat; the author considering it to be by far the most fertile cause of all diseases of the brain in old age. 2. *Meningitis*. 3. *Cerebral Softening*, under which head the physician will find a highly valuable and practical description of this important disease. 4. *Hemorrhage from the Membranes*. 5. *Sanguineous Infiltration of the Brain*. 6. *Cerebral Hemorrhage*. The first part of the volume is concluded with a special chapter on the *Treatment of Diseases of the Brain*.

The second part contains an equally full account of the special features of Pulmonary Catarrh, Pneumonia, Phthisis, Sanguineous Congestion and Infiltration of the Lungs, and Pleurisy as they occur in advanced life, together with the peculiar indications for treatment which then exist. It does not perhaps abound in so much original investigation as that devoted to diseases of the brain; nevertheless, it is most ably and comprehensively written. The same remark applies to the two following parts on the diseases of the circulation and of the digestive organs; and to the appendix, which, as already remarked, contains the essays on gout and on diseases of the skin.

On the whole, the work is a timely addition to the existing treatises on the several branches of the practice of medicine. It is well and clearly written; perhaps a little too diffuse in parts, but this is a fault common to most French and German medical authors; and as a book of reference for the practitioner or student, leaves little to be desired, if information be sought for on the diseases of old age.

Practical Observations on Gout and its Complications, and on the Treatment of Joints stiffened by Gouty Deposits. By T. SPENCER WELLS, Fellow of the Royal College of Surgeons of England; Member of the Royal Institution of Great Britain; late Assistant Surgeon in Malta Hospital, &c. London: John Churchill. 1854. 12mo. pp. 288.

THE design of the present treatise, according to the author, is not to furnish a systematic work on gout, but to institute an inquiry into certain points connected with its pathology and treatment, which he considers are not to be met with in other productions. As an example of this he alludes in his preface

to a connexion between gout and certain forms of secondary syphilis first suggested to him by the late Dr. Robert Williams, of St. Thomas' Hospital, London, and the value of the iodide of potassium as a remedial agent, "by the solvent action which it exercises upon the salts deposited round the joints in gouty persons." From the results which followed his observations on this subject, he was led to extend his inquiries to gout uncomplicated by syphilis, having had favourable opportunities for forming his opinions by a residence in Malta Hospital, as one of its resident medical officers, for five years; besides seeing numerous cases of rheumatic gout sent to that island from England, for change of air, he made a series of observations upon the relative effects of medicine and climate as far as they can be distinguished. Upon this collection of facts is formed the present work, which he offers to the profession with diffidence, as having grown out of a sincere desire to convey useful information resting on truthful data.

Treatises of this kind, not professing too wide a scope, but worked out by close reasoning and patient observation, will be always received with respect, and in his interesting little volume, the author has endeavoured to give his readers correct information rather than to display extensive reading. Without further preface we shall consider how far he has succeeded in attaining his object.

The writer begins his work with some general considerations upon the nutrition of the body and the causes which derange it. In investigating these causes, he finds that they all tend to one result, viz.—an alteration in the state of the blood. The experiments of modern chemistry have shown that in the blood of gouty persons certain principles exist which are not formed in that of healthy individuals. Further confirmation of this has been obtained by an examination of the secretions and excretions, the deposits round joints and along tendons, all of which contain the identical elements found as morbid ingredients in the blood. The natural conclusion, therefore, deducible from this is, that the blood rids itself of its impurities at the expense of other parts of the system, thereby producing disturbances of their functions and lesions of their structure. The immediate origin of these blood-changes is often doubtful, whether proceeding *ab intra* from the retention of effete tissues, or *ab extra* from imperfectly assimilated matter introduced into the circulating fluid.

The examination of the blood in gout has had encouraging, though as yet insufficient results. Thus, calcareous particles were noticed long ago by Haller as visible to the naked

eye; lithate of soda was found by Garrod and Bence Jones; urea has been also detected; these agents continue in the blood for a greater or less time, acting as sources of irritation, but eventually are removed by the emunctories of the body, or deposited from the current of the blood in various situations. The kidneys and the skin are the great purifiers of this fluid, lithic acid and the lithates being strained from the blood by these organs when their action is unimpeded; if, however, after repeated attacks of gout, they become debilitated by over-action, the balance of power in the system is disturbed, and the morbid matters left in the blood begin to be deposited in abnormal situations; hence arise those concretions of saline and earthy matters found in the fibrous tissues, destroying their elasticity and rendering them rigid and incapable of action. Chemical analysis has shown these to vary among themselves, but they contain more or less of the urates of soda, potash, and lime, with phosphoric acid and the phosphates. Such are the results supplied by chemistry, and with these facts the writer proceeds to consider the *causes* of the derangements of the processes of nutrition which give rise to those chemical changes.

In exploring the causes which deteriorate the blood in gout, a long train of hostile agencies passes in review before us, each of which possesses a greater or less influence in impairing the general health. Thus we may enumerate sedentary habits, feeble digestion, over-stimulating food, intemperance, excessive study, confined and unwholesome dwellings. It would be easy to add to this catalogue of constitutional agencies which prepare the ground for the seeds of the disease to take root. But most of them may precede other dispositions as well as the gouty, and may develop tubercle in a strumous constitution as readily as gout in a gouty one; it is necessary, therefore, to particularise with more exactness those causes which exercise a non-direct influence on its production.

One of the earliest and most powerful of these causes is "a *hereditary tendency* to particular modifications of the vital processes." There certainly is no aphorism in medicine so universally received as the tendency of parents to transmit certain peculiarities to their offspring. From the creation to the present day it has remained true that parents have produced children "in their own likeness." It would then be absurd to argue that in the lineaments of face and figure a resemblance can be transmitted, while a corresponding similitude in the shape and texture of internal organs is not congenital. Sir James Clark has fully developed the truth of this principle in phthisical habits, viz., "that a parent la-

bouring under tuberculous cachexia entails upon his offspring a disposition to the same affection, proportioned in general to *the degree of the disease under which he is labouring.*" Thus, if the malady prevail in one or both parents at the time of conception, the issue will possess a greater degree of susceptibility to the disease than children born under more favourable circumstances. If this be true when the ailment is produced by casual attacks, it becomes much more certain when the tuberculous cachexia is hereditary, though the good health of one parent may to a certain degree counterbalance the vicious condition of the other. Our author notices the same undesirable inheritance as transmissible from gouty parents, so that gout accidentally developed in an individual may descend from generation to generation through ages yet unborn, until the original gouty ancestor is enveloped in the obscurity of time. He gives some cases which illustrate this matter.

A gentleman was one of a family of six children, four of whom were born before the father had experienced a gouty attack; between the birth of the fourth and fifth the father had acute gout, and died of the disease some years later: the four elder children escaped gout, but the two younger were both gouty. These hereditary influences, if traced carefully, are as curious in a political as in a physiological light: they would form a singular chapter in the history of population. Men too often care little how much their vices and selfish indulgences may affect their posterity, no more than they care how they may react on themselves at the close of life. Still the wheels of society must roll onward; we cannot by either moral or legislative enactments stop marriages between cachectic individuals on the ground of producing a defective progeny.

In connexion with hereditary predisposition to nutritive derangements as a cause of gout, the author discusses the question whether gout can be induced by an "undue or irregular development of certain parts of the nervous system," and he combats the opinion that gout can be developed in healthy individuals by the excessive use of over-stimulating food. He believes that when hereditary proclivity is absent, this measure would only succeed in "men endowed with a highly organized condition of the nervous system." We are not disposed to go as far as the author in the adoption of this opinion. Gout, according to him, is peculiarly a disease of the high born, "distinguished for an ancestry rendered illustrious by high thoughts and noble deeds, for their own keen intelligence, for the assistance they have afforded to improvements in arts, sciences, and agriculture, and for the manner in which they

have led the spirit of the age." He continues, "I have never met with a real case of gout in other classes of the community, in a person not remarkable for mental activity, unless the tendency to gout was clearly inherited." Now an objection may here be naturally started, how it happens that gout prevails so much in an opposite class, in persons not illustrious by birth or distinguished for intelligence; for instance, in butlers, and coachmen, and hall-porters, those pampered menials who assume the habits with the livery of families of wealth and fashion. As this class has no pretension to the above-mentioned aristocratic transmission of the disease, we might perhaps be disposed to adopt the theory of Falstaff explaining the similarity of Justice Shallow and his servants, "that wise bearing or ignorant carriage is caught as men take diseases, one of another." Our author, however, contends that the disease in this servile class does not arise from similarity of habits, but is clearly inherited, the sons of these retainers of high families filling the easy station, leading the idle life, and using the same gluttonous indulgences of their fathers before them, so that master and man differ in the nosological variety of the disease, much as port wine, the drink of the one, differs from muddy beer, the beverage of the other. Whether because great men have gout, gouty men must be great men, is a proposition which certainly may give comfort to some gouty sufferers of aspiring minds, but experience shows that it cannot be relied on. We must take some exception, also, to another point of our author, wherein he asks "whether it be reasonable to suppose that a father and mother descended from persons who had cultivated their minds, and thereby kept the delicate fibrils of the brain in regular exercise, and who themselves continue to perfect the organism they have inherited, by a continuance of the same habits of mental culture, will impart to their children a brain precisely similar in size, composition, and arrangement, to that of children whose progenitors have lived in a state of mental sloth, who have never exercised their brains, and have, consequently, never attained that perfection which use imparts to every organ of the body. Such a supposition would be absurd. It would imply imperfection in the grand scheme of creation, denial of the greater perfectibility of the species, disbelief in the sure and certain progress of advancing civilization, and discourage those who are most earnestly labouring in the cause of education and humanity." Without denying that there is some probability in the above hypothesis, and that the poetic idea, "*fortes creantur fortibus et bonis boni*," is not a fiction gracefully put forward to please a patron, but a physiological fact,

we need only enter our hedge schools to find the children of some of those brainless peasants exhibiting a degree of acuteness which sometimes contrasts unfavourably with the progress of the noble scions of aristocratic houses, though possessing every advantage of education. How often do we find the offspring of eminent men degenerate! Doubtless, education and example have as much influence in developing mind as the "vis insita," otherwise courage and virtue would be matters merely dependent on the relative proportions of the brain.

Another and a most frequent cause of gout from deranged nutrition, assigned by the author is to be found in the use of food of an improper kind, or imperfectly assimilated. Experience has shown that too abundant or too rich an animal diet, if not worked off by habitual muscular exercise, tends to produce gout; and the same is true of alcoholic liquors, though of these, the vinous and fermented are more instrumental in causing gout than the distilled liquors. These principles seem to act by accumulating lithic acid in the system, but where this morbid agent is formed is as yet undecided; whether by the kidneys, the blood allowing some of it which is not excreted during sanguification to return by the renal veins to the general circulation, or by a prior process either in the changes undergone by the aliment, or in the extreme capillaries of the tissues. The author attacks the theory of Liebig upon the origin of gout from an excess of the non-nitrogenous principles in the body. These, Liebig states, attract to themselves the supply of oxygen, and thus prevent the conversion of the insoluble lithic acid into soluble urea and carbonic acid, so that it remains in the blood and appears in the urine. Mr. Wells objects to this, that a diet consisting of non-nitrogenous substances would thus increase the formation of lithic acid, which is contrary to experience, such an increase accompanying an animal or nitrogenous diet. He leans to the opinion of Prout, that lithic acid and urea are formed from the nitrogenized elements of disintegrated tissues and the imperfect assimilation of highly nitrogenized food. The results of practical observation accord with this view.

In his second chapter the author proceeds to distinguish between gout and rheumatism, which, though consanguineous diseases, have points of difference too strongly marked, in his opinion, to allow them to be regarded as varieties of a common malady. He enters into these distinctions at length, but we can only make a brief allusion to them. Thus the blood-changes are not the same in both. In gout lithic acid accumulates; in rheumatism lactic acid. Again, the fibrous tissues in gout be-

come incrustated by saline or calcareous deposits, none of which occur in rheumatism. The secondary differences are no less marked. For example, the liability to pericarditis, so frequent in rheumatism, is not seen in gout. In the former, acid perspirations are common; in gout the skin is dry. Rheumatism arises from changes of weather suppressing the secretions from the skin; gout arises more from dyspeptic causes. Rheumatism affects more the large joints; gout the small ones. The effect of remedies also may offer some specific marks of distinction, especially of colchicum, so powerful in gout, and so little available, according to the author, in rheumatism.

The author gives some cases of what is called rheumatic gout, popularly considered a kind of mongrel disease, with characters derived from both gout and rheumatism. He thinks both diseases may exist in the same person, yet preserve a *separate* course. For illustrations of his views we must refer to the work itself.

We find in the third chapter some original ideas upon gout modified by syphilis. It has lately been allowed that the poison of syphilis remains for years in the blood, and that it can be transmitted long after the original contamination, from parent to child, and in all probability from a diseased child to a healthy nurse. Now the existence of the syphilitic poison in the blood most probably modifies other diseases subsequently occurring, especially a blood disease like gout. In this way, some anomalous symptoms, apparently in connexion with gout, are more properly due to syphilis. Of these he cites examples of purpura, of lichen, and other cutaneous eruptions, of affections of the joints, &c., considered as gouty, but amenable to the treatment of syphilis.

We had marked some valuable observations in the fourth chapter on the morbid anatomy of gout in its structural changes, and in the fifth chapter on gout in the female, but we must leave to our readers the pleasure of perusing them for themselves.

The therapeutics of gout forms an extensive subject, about which much has been written; and our author divides its consideration into what he calls the *natural* and *artificial* treatment. The first, which we consider rather a doubtful expression, and which may better be replaced by the word *hygienic*, embraces the points connected with diet and regimen, a very useful chapter being devoted to these matters. Before entering on the medicinal treatment he discusses the cold-water cure, which has a chapter to itself, where he places it *between* diet and medicine, and calls on the physician to lay aside prejudice and em-

brace all that is useful in this agent. Mr. Wells, who certainly appears unbiassed in this matter, considers the hydropathic treatment dangerous in acute gout, and in chronic gout, when the amount of constitutional depression is considerable, which he thinks can be tested by watching the degree of reaction which ensues on sprinkling freely cold water over the surface of the body. In purely chronic cases, where a healthy reaction follows the free application of water followed by friction, he thinks it useful; and it may be applied by the wet sheet and local wet compress covered with oiled silk; the local douche is also often useful in stiff joints. The quantity of water to be drank he leaves to the instinct of the party, who should take care not to overdo the matter. In summing up the whole he comes to a kind of compromise with the medical practitioner, allowing all these measures to be done at the patient's house under his guidance.

In the medical treatment of gout, the author is eclectic. He justly condemns blood-letting, though in some gouty complications local bleeding is useful. Purgatives should be used with caution, and the warmer kinds preferred. He discountenances the employment of sudorifics, thinking their effect not beneficial, and prefers, where perspiration is required, that the hot air or vapour-bath should be employed instead of internal sudorifics. He gives easy and simple directions for obtaining either of these baths at little trouble. His observations on diuretics are valuable, and will repay perusal.

Attention has lately been drawn to chemical solvents of uric acid, and he prefers the Vichy water, or Struve's imitation of it, to any other. He gives in detail the various kinds of solvents recommended by authors, but regards the iodide of potassium as the most useful, preferring small to large doses. This remedy is especially indicated in combinations of gout with syphilis. The value of colchicum as a specific he discusses at length, and gives the preference to the *tincture of the flowers* as the best preparation; ten minims of this every three hours in an acute attack, and the same two or three times daily in chronic gout, being as much as he deems necessary. In long attacks, doses of a homœopathic amount, such as one or two drops, with similar subdivisions of a grain or two of the iodide of potassium, he regards as exercising a slow but certain influence both in preventing and modifying the results of the disease.

We must here conclude our notice of Dr. Wells' treatise, from which we have received both information and pleasure. We recommend it to our readers as containing much valuable informa-

tion on a disease of great interest, especially to such as practise in large cities. Both in its pathology and therapeutics they will find much that is new, and more that is useful. The principles of treatment which he lays down are the result of experience, guided and applied by correct physiological views.

The Science and Art of Surgery, being a Treatise on Surgical Injuries, Diseases, and Operations. By JOHN ERICHSEN, Professor of Surgery in University College, and Surgeon to University College Hospital. London: Walton and Maberley. 1853. 8vo, pp. 952.

IF he only can teach anatomy efficiently who has studied it carefully in the dissecting-room, or chemistry who has worked assiduously in the laboratory, assuredly none can give sound instruction in surgery or medicine, unless he who has learned disease from hospital practice.

The large surgical work before us has the recommendation of coming from the pen of an hospital surgeon, and it is gratifying to find that, where energy exists, a great undertaking like it can be achieved, notwithstanding the many disturbing circumstances which beset the private practitioner in a large metropolis.

We have often remarked that there is no more difficult task than to write a complete work on the theory and practice of surgery, and indeed, to suppose that a book embodying such a variety of material could be equally good in every point, and free from error in any, would be almost impossible; we are bound, however, to state, and we do so without wishing to draw invidious comparisons, that the work of Mr. Erichsen, in *most respects*, surpasses any that has preceded it. Mr. Miller's book on the Principles of Surgery reflects the highest credit on the author's learning, and so far as the theory of disease is concerned, may be considered as unequalled in fulness, clearness, and faithfulness of description. Mr. Erichsen's is a practical work, combining a due proportion of the "science and art of surgery," and certainly reliance may be placed on its statements regarding the leading points of practice. Some subjects are not as fully nor as satisfactorily dealt with as we should desire, but, as a whole, the imperfections of the work weigh very lightly.

The volume is separated into three divisions. The first treats of "first principles,"—inflammation, operations in general, amputations and disarticulations; the second of "surgical

injuries;" the third of "surgical diseases." We shall pass by the first part, and dwell on the second.

"*Effects of Injury.*—The effects of an injury, if at all severe are twofold,—constitutional and local. The constitutional effects are immediate and remote."

There is nothing to which the surgeon should pay more attention than to the effects of injuries. Most injuries, and we may say all surgical operations, are rarely in themselves fatal, their seriousness arises from the chance that some of the evils included in the above general division may supervene. One of the most serious of the remote effects is traumatic delirium: of it the author gives the following concise description:—

"*Traumatic delirium* not unfrequently occurs in cases of severe injury in individuals with an irritable, nervous system; particularly in those who had been drinking freely before, or were intoxicated at the time of the accident. It usually comes on about the third or fourth day, but not unfrequently earlier than this; and most commonly declares itself during the night. This disease presents two distinct types—the one inflammatory, the other irritative.

"In *inflammatory traumatic delirium* there is a quick and bounding pulse; hot skin and head, flushed cheeks, glistening eyes, much thirst, and high febrile action generally. The delirium is usually furious; the patient shouting, singing, tossing himself about the bed, and moving the injured limb regardless of pain. The treatment of this form of the disease is strictly antiphlogistic. Bleeding from the arm, or leeches and ice to the head; purging, and low diet will subdue it; but in many cases it is speedily fatal.

"The *irritative, or nervous delirium* usually occurs in persons of a broken constitution; and closely resembles ordinary delirium-tremens. In this form of the disease the pulse is quick, small, and irritable; the pupil dilated; the surface cool; the countenance is pale, with an anxious, haggard expression, and bedewed with a clammy sweat. The tongue is white, and there is sometimes tremor of it and of the hands; but this by no means invariably occurs. The delirium is usually of a muttering and suspecting character; the patient is often harassed by spectral illusions, but will answer rationally when spoken to. This form of disease is sometimes very rapidly fatal. A strong, hale man, but an immoderate spirit-drinker, admitted into University College Hospital with a simple fracture of the thigh, died of this form of delirium twelve hours after it first showed itself.

"The *treatment* of irritative traumatic delirium consists essentially in the administration of opium until sleep is procured, or the pupil becomes contracted. For this purpose large quantities are frequently required; and the drug should be given in full doses; from twenty to thirty minims of laudanum or half a grain of the muriate of morphia being required every second or third hour.

"If there be much depression, it will usually be expedient to administer the opiate in porter, or in that stimulant to which the patient—if a drunkard—has habituated himself. The administration of the opiate should be preceded by a free purge and an aperient enema, so that all source of irritation may be removed from the intestinal canal. A strait-waistcoat is commonly necessary in all cases of traumatic delirium, in order to prevent the patient injuring the wounded part.

"After sleep has been induced, the quantity of the opiate must be lessened; but it will be found necessary to continue it for some time, as there will be a tendency to the recurrence of the delirium at night.

"These two forms of traumatic delirium, the inflammatory and the irritative, are often found more or less conjoined; in practice a modification of the treatment then becomes requisite,—the surgeon depleting with one hand, and allaying irritation and giving support with the other."

The next chapter treats of injuries of soft parts—contusions and wounds.

In speaking of contused and lacerated wounds, the author observes—

"But the chief danger to be apprehended in wounds of this description is the supervention of gangrene. In these cases gangrene may occur in three ways:—

"1st. The contusion always kills a thin layer of tissue, which forms a slough on the sides or lips of the wound; but, in some cases, the violence done to the part is so great as directly to kill its whole substance.

"2ndly. The injury may be chiefly inflicted upon the great vessels of the limb, damaging them to such an extent as to interrupt, completely, the circulation; gangrene being thus induced, indirectly, in the parts supplied by them.

"3rdly. The true traumatic or 'spreading gangrene,' the most fatal variety of mortification, may occur from comparatively slight wounds, in consequence of some constitutional disorder; but most commonly it is the result of severe contused and lacerated injuries and fractures.

"The peculiarities of this form of gangrene consist in the rapidity of its progress, its great fatality, and the tendency it has to affect and spread along the cellular tissue of the limb in preference to any other part. It is truly a constitutional affection, depending rather on the state of the blood than upon the local injury. Thus in some cases we see it follow comparatively slight contused wounds, whilst in others the most serious crushes and injuries may occur without it. It appears to be closely associated in its occurrence with those conditions of the system that dispose to the supervention of erysipelas, phlebitis, and other inflammatory diseases of a diffused or spreading character; and hence its great fatality. From the

other varieties of traumatic gangrene it differs remarkably, not only in the rapidity of its extension and the general appearance presented by the parts affected, but more especially in the little disposition it manifests to limit itself by a line of demarcation or by any adhesive action."

The great practical point connected with traumatic gangrene is the question as to the admissibility of amputation. Whether Potts' doctrine, that we should wait until the line of demarcation has formed, or Larrey's advice to disregard this rule, should be followed, has, for a long time, been made matter of active controversy. Mr. Erichsen says:—

"In all cases in which the true traumatic or rapidly spreading gangrene has set in, the surgeon will be placed in a great difficulty whichever way he act. If he leave the patient to nature, in the hope of a line of demarcation forming, he will almost certainly be disappointed, the gangrene rapidly spreading up to the trunk; and if he amputates, he will probably lose his patient by the stump becoming affected. Yet amputation should, in my opinion, be performed at once."

This latter observation requires qualification. To afford any chance of success there is no doubt that the earlier amputation is performed the better; but we cannot in all cases operate early in one sense, that is, before the gangrene has spread very far; for sometimes its extension is so rapid that an entire limb will become involved in a few hours. The rule which should guide us in determining whether the operation ought to be performed or not should have relation not so much to the time the gangrene has been in existence, as to the character of the accompanying symptoms. The fact that this is a constitutional disease demands the greater regard to symptoms. It is not then the extent nor the duration of the gangrene we are to consider, but the general condition of the patient; this is such as rarely to afford the operation any prospect of success.

Injuries of Arteries; Hemorrhage.—Can anything be more beautiful or better calculated to impress the mind with the wonderful and beneficial designs of Providence than the contemplation of the method to which nature resorts for the arrest of arterial hemorrhage? The very circumstance which excites the fear of the individual himself, or the bystander, the super-vention of weakness or fainting, is what most of all facilitates the means by which a temporary check is given to the bleeding, and which changes must occur before those which terminate in the complete occlusion of the vessel. The series of changes which take place in a large artery when divided transversely, from its retraction and contraction to its final obliteration—

tion, are truly wonderful, and afford a most attractive study to him who loves to investigate nature's laws. But it is not the physiological interest which attaches to this subject which is alone to be considered; the knowledge derived from the study of it has led to most important practical results. To Jones is undoubtedly due the credit of having clearly demonstrated most that is known of the laws which regulate the arrest of arterial hemorrhage; having, by a well-conducted set of experiments on animals established the true changes which take place from first to last, he cleared away all previous erroneous notions.

Mr. Erichsen gives a very clear and succinct account of the natural and artificial means of arresting hemorrhage, bringing before the reader in a small compass a considerable amount of matter.

Traumatic aneurisms which are so closely associated with the subject of hemorrhage are also described, and wood-cut representations are given of that very interesting form of varicose aneurism. The author very properly shows the difficulty attending on the operation for the cure of varicose aneurism. Fortunately however, the operation is seldom called for, and we feel quite convinced that, unless from some circumstance it is positively demanded, it ought not to be attempted. We can refer to the case of a man who has had an aneurism of this sort in the left thigh for upwards of ten years, the tumour having remained stationary, and not interfering with his taking exercise in any respect.

Fractures.—In speaking of compound fractures Mr. Erichsen observes:—

“Those fractures must be looked upon as most unfavourable in which the wound is the consequence of the violence that breaks the bone, and in which there is much laceration of, and extravasation into, the soft parts; more particularly if the integuments are stripped off, portions of the muscular bellies protruding, and the planes of cellular tissue between the great muscles of the limb torn up and infiltrated with blood. Injuries of this description occurring in the lower extremity always require amputation. In the arm, they are not so serious, and admit of the member being saved, unless the bones be greatly comminuted.”

We can only subscribe to this statement to a certain extent. It is true that if the soft parts be extensively torn, and the cellular tissue much infiltrated with blood, the case is bad, and always demands amputation. But it is necessary to draw a line of distinction between those compound fractures, which

must be condemned at once, and those where it is proper to endeavour to save the limb. In the latter cases it is not always those in which the integuments at least are much injured, which are apt to take the most unfavourable course; on the contrary, our experience leads us to consider that dangerous results are far more prone to occur where the wound is small, and particularly if it were produced by protrusion of the bone. Where the skin has been freely torn, or extensively injured by direct violence, the case for some days wears a very ugly aspect, but as soon as the sloughs have separated, healthy granulations spring up, and though the sore thus formed is often very extensive and attended with much suppuration, it is sometimes wonderful how rapidly the healing process is effected. With respect to secondary amputation in compound fracture, the author says:—

“*Secondary amputation* may become necessary from the occurrence of traumatic gangrene, when it must be done in accordance with the principles already laid down when speaking of that operation; but more frequently it is required from failure of the powers of the patient in consequence of irritative and asthenic fever, induced by the general disorganization of the limb, or by hectic resulting from profuse suppuration and slow necrosis of the bones. Under these circumstances, the constitution suffers from the local irritation which is the source of the wasting discharge, but by removing this in time, and seizing an interval in which constitutional action may have been somewhat lessened, the patient's life will in all probability be preserved; the results of secondary amputation for compound fracture under these conditions being by no means unfavourable. Indeed, it is remarkable to see how speedily the constitutional irritative and hectic symptoms subside after the removal of the source of irritation, the patient often sleeping well, and taking his food with appetite the day after the operation.”

Space will not permit our dwelling upon the other subjects in the second division of the work,—injuries of the head and spine, chest and abdomen. The observations on each of these important points, though short—unsatisfactorily short, in some instances—are based on sound practical principles.

Third Division.—This part of the work commences with the subject of abscess:—

“Surgeons divide abscesses into various kinds according to the symptoms attending them, their duration and cause. Thus they speak habitually of *acute* and *chronic*, *hot* and *cold*, *lymphatic*, *diffuse*, *metastatic*, and *puerperal* abscesses.”

The diagnosis of abscess is perhaps one of the most important points in practical surgery, and as no acquirement is more advantageous than acute perceptive powers in this respect, so there is none which should be more eagerly sought. Nothing so much displays the superiority of the well-educated and sagacious surgeon as the possession of this much-to-be-desired qualification. The capability of detecting deep-seated matter is of the utmost importance in the practice of surgery, and to accomplish this with quickness and precision must be viewed as a high perfection. The tactile sensibility necessary for such a purpose is possessed naturally by some, but almost all can, by practice, acquire it, and even those who are gifted with it by nature, should endeavour to refine it by education. The "tactus eruditus" has ever been considered as a qualification of the first order, and certainly, when the character of the surgeon's practice is considered, it becomes evident that without the possession of it his diagnostic capabilities must be of rather an inferior grade; and as regards many operative proceedings, a want of the necessary manual dexterity must be experienced. The author alludes to the fact that abscesses are apt to be confounded with solid elastic tumours. How often have malignant tumours been mistaken for chronic abscesses? Many a time has a lancet or trocar been plunged into a mass of cerebriform cancer, owing to the deceptive feel conveyed by the elasticity of the substance. The more acute the tactile sensibility, the less the chance of being led astray by delusive sensations. There is one natural source of deception not alluded to by the author, and to which it is necessary to pay particular attention, viz., the many sensations produced by the rolling about of muscles beneath the hands. In some situations, for instance, on the anterior part of the thigh, when alternate pressure is made on the muscles, a sensation is yielded, which is often with difficulty distinguishable from that of fluctuation. The error is very apt to be made in the gluteal region; matter is often declared to exist there when not a drop is present.

In disease of the hip-joint the tissues in this quarter become infiltrated, so as to produce considerable swelling, and there is, at the same time, tenderness on pressure; a deep-seated abscess, accordingly, is suspected, fluctuation is sought for, and the deceptive feel alluded to at once leads to a wrong conclusion.

"The *treatment* of abscess presents three points requiring attention. The first object should be to prevent the formation of matter; the next to take steps for its evacuation when formed; and, lastly, to endeavour to close the cavity that results."

The second point is chiefly to be considered with respect to chronic abscesses. The rule as regards the opening of acute abscesses is simple enough; not so the chronic,—whether they are to be opened at all or not,—if they are, at what period, and how it is to be done, are points of the greatest practical importance, and upon them the greatest discrepancy of opinion prevails. We regret the author has not recorded his views on this part of the subject.

A short account being given of ulcers and mortification, the next disease introduced to our notice is erysipelas. The great importance of this subject, in a surgical point of view, imposes a greater duty on the author's pen. He adopts the division of erysipelas laid down by Mr. Nunneley as the best, viz., into the "cutaneous, cellulo-cutaneous, and cellular:"—

"The *cellular erysipelas*, or, as it is often termed, *diffuse inflammation of the cellular tissue*, or *cellulitis*, is a disease that has been particularly described by Duncan, Arnott, Lawrence, and Nunneley. It always arises from a wound or injury, often, however, of an apparently trivial character, and most commonly affects the sub-cutaneous cellular membrane, though occasionally it extends to the sub-aponeurotic tissue, and then is a more severe and dangerous affection. Though commonly arising as a consequence of ordinary injuries, it is especially apt to follow those in which there has been any inoculation of animal poisons, as in dissection wounds, the stings of insects, and the bites of venomous reptiles. In whatever way arising, it is characterized by the rapidity and extent of the sloughing of the affected tissue, and by great depression of the powers of the constitution. That the diffuse inflammation of the cellular tissue, whether it is limited to a finger, or implicates the cellular membrane of half the body, is a variety of erysipelas affecting this membrane primarily, and the skin secondarily, there can be no doubt. The points of resemblance between cellulitis and erysipelas have been well shown by Nunneley. Not only are the local effects precisely the same in the two diseases; the same swelling, tension, infiltration of pus, and formation of gangrenous shreds and sloughs, but the constitutional symptoms, though differing perhaps in degree, present no variety as to character. The results also are identical, there being the same impairment of structure locally, the same tendency to involve parts at a distance, and to the formation of secondary abscesses. So also these two diseases occur in the same constitutions, in the same states of the atmosphere, and in the same situation; one form of disorder may produce the other, and, lastly, the same treatment is required for both affections."

The author's observations upon the treatment of this disease are well worthy of attention. The plan of employing depletion, which was formerly so much resorted to, he very pro-

perly dissents from. He says, "the apparent intensity of the inflammation should not lead the surgeon into the fatal error of employing an over-active antiphlogistic treatment." In truth, there are few cases of erysipelas to be met with which will even bear the withholding of stimulants, not to say the adoption of antiphlogistic measures; and in bad cases, accompanied by a very low asthenic state of system, the only chance of recovery is, by pouring in wine and brandy as freely as possible. Perhaps no greater improvement has taken place in the treatment of disease than the administration of wine in inflammatory diseases of a certain character, and the recognition of this principle has been of no less value in erysipelas than in fever. Formerly the idea of giving wine where inflammation existed would be viewed as an irrational, practical, and physiological contradiction, and this is natural enough, seeing that the disease was viewed only as one of plus vitality. Since, however, physiological research has established that inflammation possesses two distinct types,—that genuine inflammation may exist with diminished as well as with increased vitality, the advantage of wine, as a remedial agent, has been fully recognised. More lives, we are confident, have been saved by the free use of stimulants in the asthenic types of inflammation than have been lost by the non-adoption of depletory measures in the opposite forms.

The next subject treated of in the work, to which we shall direct attention is Aneurism. One chapter is devoted to the pathology, diagnosis, causes, and treatment of aneurism in general; the other, to the special forms of the disease, including a description of the operation for deligation of the main arteries:—

"When the arterial walls have undergone more or less fatty degeneration, whether that consist in the distinct deposit of atheroma, or in a sort of molecular deposit of fat globules in the tissues comprising their coats, their natural elasticity and resiliency become lost, proportionately to the amount of fatty change that has taken place within them. Hence as the artery becomes less and less able to contract on its contents, and to recover during the diastole the tension exercised on its walls during the systolic impulse, it gradually becomes distended by the repetition of the shocks which it sustains, and thus either complete or partial dilatation of its cavity takes place. I believe that this loss of elasticity and of power of contracting on its contents, which eventually results in the dilatation of the vessel, never occurs except as the result of previous disease of the coats. In the very numerous specimens of dilated arteries that I have examined, I have never found one that had not undergone fatty degeneration, or atheromatous deposition. Calcification, on the other

hand, rather prevents dilatation of the artery, by hardening the coats and converting them into rigid inelastic tubes; but atheroma softens them, and causes yielding of that portion of the vessel affected by it. I have frequently observed that the whole of the artery may be healthy except at one part, where there was an atheromatous patch, and that there the vessel was dilated; or that the whole of its coats might be calcified except at one spot, where atheroma was deposited, and where consequently the coats had yielded under the outward pressure of the contained blood. This general or localized dilatation of the arteries is termed *aneurism*, an affection that is, I believe, invariably dependent upon the coats having been softened, atrophied, and disintegrated by fatty degeneration, and consequently yielding to the eccentric pressure of the contained blood."

With respect to the "true sacculated aneurism," the author says,—

"The existence of *true sacculated aneurisms* has been denied; thus, Scarpa doubts the occurrence of such a disease, and Bizot seems disposed to coincide with him. With these eminent pathologists, however, I cannot agree, and though I am willing to admit that many of the so-called true aneurisms are not so in reality, yet I cannot doubt from repeated observation that Hodgson is right in saying that in their early stages aneurisms are not unfrequently of the true kind. Thus, we occasionally meet, as Dr. Peacock has pointed out, with small digital pouches springing from the walls of some of the larger arteries, through the whole of which the external, middle, and internal coats can be demonstrated by maceration to exist; and in those aneurisms which are formed by the dilatation of a comparatively large portion of the arterial wall, it not unfrequently happens that the tumour remains of the true kind for some time, as I have had occasion more than once to ascertain by careful dissection. But after an aneurism has attained a certain size, its coats become so fused together, and so closely incorporated with the neighbouring tissues, that their precise structure cannot be made out. Indeed, for a sacculated aneurism to be of the *true* kind, I believe that two conditions are necessary: 1st. That the tumour itself be small; and, 2ndly. That the mouth of the sac be of tolerably large dimensions. Porter says, that he has never met with a true aneurism larger than a small orange, and, certainly, none of those that I have seen, provided they were of the sacculated kind, have exceeded this size. In true sacculated aneurisms, also, it is necessary that the mouth of the sac, or that portion of it which communicates with the interior of the artery, should be of a good size, and not bear too great a disproportion to the wall of the tumour. I cannot conceive a large sac with a small mouth to be a true aneurism, for, as the mouth of the sac corresponds exactly in size to that portion of the arterial coats which have been originally dilated, it is not easy to understand how a large sac can be expanded out of a small segment of the wall of

the artery; though, as in all cases of true aneurism, however small they may be, the size of the sac greatly exceeds that of its mouth, it is clear that there must have been, not only expansion, but a degree of hypertrophy and over-growth of the wall of the vessel, just as in the tubular aneurisms."

We think there can be no doubt of the existence of a form of aneurism in which the sac is composed of all the coats of the vessel. We have in our possession a specimen of popliteal aneurism which affords a beautiful example of the true kind; all the coats can be distinctly traced from the vessel to the sac.

With respect to the treatment of aneurism by compression, it is gratifying to find that the author deals fairly and candidly with the subject.

Though this great improvement in modern surgery has not met with the decided opposition in England that it has in Scotland, still it has not, even at the present moment, taken a proper stand in the former country. It is true, that many cases of cure by compression have been reported from various parts of England; still it does not appear to have gained the full confidence of the profession. In Ireland, where the adoption of this plan originated, numberless cases have been cured by it; hardly a week passes by without the occurrence of some successful case to swell the statistical Table, so that not the smallest doubt can be entertained of its perfect adequacy to effect a cure, while its advantages over the ligature must become evident. It may be argued that the statistics of this method of treatment in England do not warrant the same conclusions; then we naturally inquire why this dissimilarity as to results should exist? To what cause is attributable the failures in the sister country? The answer is twofold,—first, all the details of the plan are really not yet perfectly understood;—second, there still exists, as it were, a latent prejudice against, or want of confidence in it, as a curative method, which prevents that degree of attention being devoted to it which is essential to the success of any undertaking; and it is only natural to suppose that if the practitioner exhibits a want of confidence in the plan adopted, the patient will not be likely to become differently inspired. If compression is to succeed, an accurate knowledge of its *modus operandi* is indispensable, combined with patience, perseverance, and constant watching. The author concisely expresses the very point which we wish to put prominently forward; he says:—

"The success of the treatment by compression depends greatly upon a scrupulous attention to a number of minor circumstances,

which, though trivial in themselves, become of importance when taken as a whole."

A description of the plan of compression is given which is as full, perhaps, as could be expected in a work of so general a character. The view taken of the relative merits of compression and the ligature corresponds exactly with our own, and is no more than was ever put forward by the advocates of the former method:—

"It should also not be forgotten that in some cases, such as when aneurism is complicated with heart disease, or occurs in a very broken and unhealthy constitution, in which the operation necessary for the application of the ligature would scarcely, or not at all, be admissible, compression may be safely employed."

The following observations on the diagnosis of carotid aneurism are worthy of attention:—

"From abscess of the neck the diagnosis must be made on general principles; the co-existence of ill-defined hardness and of enlargement of the glands, of an inflamed state of the skin, the ready detection of fluctuation, and the absence of expansive pulsation in the tumour, will show that it is not aneurismal, however similar its other characters may be. It is also of importance to observe, that an aneurism that fluctuates is always forcibly distended with strong pulsation, and can be materially diminished by pressure; neither of which circumstances can possibly occur in abscess. But if abscess may be mistaken for aneurism, the converse also holds good, and an aneurism may, unless care be taken, be mistaken for abscess; a far more fatal error. And there is one variety of false aneurism, that to which Mr. Liston has invited special attention, against which the surgeon must be carefully on his guard, on account of the many points of resemblance between it and aneurism; I mean the case in which an artery has given way into the sac of an abscess. In this case, fluctuation and pulsation will exist, although not, perhaps, of a distending kind. An important diagnostic mark will be, however, that the outline of an aneurism is distinctly defined and limited, while that of an abscess never is. Aneurism of the internal carotid has been found by Syme to simulate very closely abscess of the tonsils."

We fear we have dwelt too long on the preceding subjects to enable us to make even a passing observation on the many important points which follow; suffice it to say, that the author fully bears out, in the subsequent pages of the work, the high opinion he earned in our estimation from a review of the former:—diseases of the bones, joints, and rectum, hernia, urinary diseases and affections of the testis, are each dealt with in

the same sound, practical manner which characterizes his description of the diseases we have particularly considered.

We now close the pages of Mr. Erichsen's work, having derived no little instruction from it in many important branches of surgery, and we have no hesitation in recommending it as a valuable book alike to the practitioner and the student.

Dictionnaire d'Hygiène Publique et de Salubrité, ou Répertoire de toutes les Questions relatives à la Santé Publique, considérées dans leurs Rapports avec les Subsistances, les Épidémies, les Professions, les Établissements et Institutions d'Hygiène et de Salubrité, complété par le texte des Lois, Décrets, Arrêtés, Ordonnances et Instructions qui s'y rattachent. Par le Docteur AMBROISE TARDIEU, Professeur agrégé à la Faculté de Médecine de Paris, etc. Paris and London: Hippolyte Baillière, 1852 et 1853. Vols. I and II. 8vo, pp. 567 and 532.

THE very comprehensive title of this work sufficiently indicates the variety and extent of the information it contains, and we need therefore scarcely say, that M. Tardieu has conferred a great boon not alone on his own countrymen, but on all in these islands engaged in the study of medicine in any of its medico-legal branches. The much higher position which State medicine holds in France, and the consequent greater advancement which it has attained, is well illustrated by a reference to any of the leading articles in either of the volumes of this as yet unfinished book. As soon as the concluding volume, which is announced for immediate publication, shall reach us, we purpose to bring under the notice of our readers some of the more important matters treated of; in the mean time we can most strongly recommend it as being complete and accurate, and as presenting a full review of the laws and history of all subjects connected with public hygiene.

PART III.

MEDICAL MISCELLANY.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

FOURTEENTH SESSION.—1853-54.

Caries of the Cervical Vertebrae.—Professor R. W. Smith exhibited a specimen of very extensive destruction by caries (most probably of a syphilitic nature) of the first and second vertebræ of the neck, taken from the body of a man about forty-five years of age, who died suddenly within a very short period after his admission into the Richmond Hospital.

The anterior arch of the atlas was carious throughout its whole extent, and its central portion, corresponding to the odontoid process, nearly altogether destroyed; its lower border, for a few lines in depth, alone remained, and this attenuated portion had given way by fracture. The inferior oblique processes were destitute of cartilage, and presented a worm-eaten aspect; that upon the right side was destroyed to a level with the remainder of the inferior surface of the bone. The tubercles which give attachment to the transverse ligament were both involved in the process of ulceration.

With regard to the second vertebra, the odontoid process, together with that portion of the bone from which it arises, remained; but all the rest of the body of the vertebra had disappeared. The odontoid process itself was superficially ulcerated and irregular in form. The left superior articular process was deeply carious, and the ulcerative action had extended to the inner boundary of the foramen which transmits the vertebral artery. Upon the right side the articular process was totally destroyed, together with the greater portion of the transverse process, so much so that upon this side, the vertebral artery, in its passage from the third vertebra to the atlas, was not transmitted through any foramen. The inferior oblique processes of the axis were ankylosed to the superior of the third vertebra, a small portion of the body of which upon the right side was carious.

The frontal, parietal, and occipital bones were diseased throughout the greater part of their extent, and their external surface presented a nodulated aspect, caused by circles of caries, isolating healthy portions of the osseous tissue; the ulcerative action had penetrated deeply, and in about thirty different situations both tables of the skull were perforated; in some places the inner table alone was destroyed, while in other situations the external only had suffered; the morbid matter which filled the different excavations seemed to have been originally deposited in the diploe, and consisted of a soft, yellow, cheese-like matter. Some of the bones of the face were also carious. A diffused swelling occupied the back of the neck, and one or two small tumours, containing caseous matter, existed upon the head. The brain and its membranes were healthy.

The sudden death of this patient so very soon after his admission into the hospital rendered it impossible to obtain any accurate history of his case. Upon the evening of the day of his admission, when he retired to bed, there was nothing remarkable observed in his condition, nor did he complain of anything more than stiffness and pain in the neck. Early upon the following morning, while in the act of turning in bed, he was suddenly seized with a kind of convulsive attack, the most urgent dyspnoea set in, his countenance became congested, his lips livid, and he died in a few minutes.

The condition of the bones of the head and face, Professor Smith remarked, induced him to believe that the affection of the osseous system, under which this patient laboured, was of syphilitic origin. That the affection of the cervical portion of the spine was of very long duration was manifest from the circumstance of ankylosis having taken place between certain portions of the opposed surfaces of the second and third vertebræ, and to the destruction of the connexion between the atlas and axis is to be ascribed the sudden death of the patient, the first vertebra, with the head, having passed forward, so as to compress the spinal marrow between the posterior arch of the atlas and the front of the spinal canal.

It is incorrect to assign the displacement of the odontoid process (as many surgical writers are in the habit of doing) as the cause of the sudden death in these cases, for the destruction of the transverse or cheek ligaments leads to no luxation of that process (the connexion of the second with the third vertebra remaining entire), but permits of the displacement forward of the atlas along with the head. Even so careful a writer as Sir Benjamin Brodie has fallen into this inaccuracy, for at page 330 of the last edition of his valuable work upon the Joints, he remarks that "not unfrequently the transverse ligament of the second (first?) vertebra is destroyed, and the consequence is a dislocation of the odontoid process. Sometimes the dislocation is complete, and the patient, from the pressure made on the spinal cord, expires as suddenly as if the latter had been divided transversely."

The escape of the vertebral artery, in the midst of so much surrounding disease, is worthy of being mentioned. A specimen was,

for many years, preserved in the Museum of the Richmond School of Medicine, of caries of the cervical vertebra, in which the ulceration having extended to the transverse processes, opened this large vessel, and gave rise to a fatal hemorrhage.

Professor Smith concluded by referring to the remarkable cases of dislocation of the atlas from disease, recorded by Mr. Paget and Mr. Shaw, in the thirty-first volume of the *Medico-Chirurgical Transactions*, and also to that of exfoliation of the anterior arch of the same bone, published by Mr. Wade, in the thirty-second volume of the same valuable work.—*January 14, 1854.*

Chronic Rheumatic Arthritis of the Hip-Joint.—Professor R. W. Smith exhibited a most remarkable specimen of this disease, which he had removed from the body of a man, aged 85, who had been admitted several months before his death into the Richmond Hospital, labouring under a chronic ulcer of the right leg, which had been recently attacked with gangrene. He was a man of very large size and powerful frame, and struggled desperately against the constitutional effects of the mortification, which before his death (which occurred a few days ago) had spread over the entire of the foot and the greater part of the leg.

During the examination of the limb, shortly after his admission, an unusual prominence of the trochanter and general deformity and stiffness of the hip-joint attracted observation, but the patient evinced the greatest unwillingness to give any information respecting its cause or history; at length, however, it was with difficulty extracted from him, that he had joined the rebel forces in the year 1798, and had fought at the battle of Vinegar-Hill, where he was wounded by a musket-ball in the thigh, below the great trochanter, from the effects of which injury he was laid up for several months, and that during the whole of this time he was concealed in a damp cave in the mountains. From that period forward he was never free from uneasy sensations and occasional pains in the hip, from year to year his lameness increased, and the stiffness of the joint became greater, until at length all the usual signs and symptoms of chronic rheumatic arthritis became fully established.

When the joint was examined after death, the anatomical characters of this disease were found to be present in an exaggerated degree; the neck of the femur was to a great extent absorbed, and the head of the bone enlarged, flattened from above downwards, destitute of cartilage, and smooth and polished; it measured thirteen inches in circumference; numerous foreign bodies, partly osseous and partly cartilaginous, hung by slender membranous pedicles from beneath its irregular outline. The acetabulum had undergone corresponding alterations of form and size; the capsular ligament was greatly hypertrophied, being in some places upwards of an eighth of an inch in thickness, and within it was contained a very viscid fluid; the ligamentum teres and the ligaments of the notch had disappeared, and the osseous structures were intensely vascular.

The disease had likewise established itself in the right and left acromio-clavicular articulations; the enlargement of the articular surfaces causing the joints to assume very much the aspect of dislocation upwards of the outer end of each clavicle.—*January 14, 1854.*

Caries of the Temporal Bone; Abscess in the Brain.—Dr. Hutton said;—"The case I am about to lay before the Pathological Society is one of abscess in the brain, connected with disease of the temporal bone. The patient was a girl, aged 16, always lively and intelligent until her present complaint commenced, with this exception, that she was subject to sick headaches, the pain in the head increasing till she vomited, and then subsiding. At first these attacks came on at distant intervals, but twelve months ago, being much exposed to cold, and frequently sleeping in damp places, they became much more violent, and recurred every fortnight, the pain occupying chiefly the right side of the head. Three months before her admission she began to grow slightly deaf, and sometimes would ask her mother if she heard a noise in her ears, and constantly said that there were figures moving before her eyes. About the same time she began to grow dull and heavy, frequently falling asleep. She was taken to a dispensary for the deafness, and was told there was a tumour in the ear, which should be removed. The liability of fungus arising from diseased bone to be mistaken for polypus of the ear, is manifest; here it was evidently mistaken for one; but, on examination with a probe, a sudden discharge of matter, mixed with blood, took place, after which the pain became much less. The discharge continued for two months, and then ceased, the pain at the same time increasing with great violence. Upon the 21st of January last, the discharge having returned, her suffering became somewhat less, but her spirits were extremely depressed; she seldom spoke, unless when asked a question, making signs for anything she wanted. Upon the 30th of January she was admitted into the Richmond Hospital; she remained all day in a half stupid condition, taking no notice of anything about her; when spoken to, her countenance assumed an expression of great perplexity; she seemed to comprehend the nature of the question, but to have forgotten the words necessary for her reply; she frequently employed wrong words, spoke always in a whisper, but distinctly and without difficulty; she referred her pain chiefly to the left eyebrow and temple; she never asked for food, and had not eaten anything for some days; she had never had convulsions, or loss of motion or sensation.

"She continued without any remarkable change in her condition until the evening of February 4, when she became very restless, working herself almost out of the bed, yawning widely and protruding her tongue. At the end of a few hours she died in convulsions.

"On removing the calvarium we found the dura mater over the convex surface of the brain healthy; the arachnoid somewhat vascular; on removing the brain some matter escaped from its base, very

fetid, and of a greenish colour. The middle lobe was fuller than natural, and its convolutions and sulci were effaced. The greenish discoloration extended over a considerable space, and the brain felt soft and yielded under the fingers, but in other parts appeared healthy. On making a section, an abscess was discovered, lined with a membrane and containing fetid green matter; it was an inch and three-quarters in length, and nearly the same in breadth; the brain around it was soft, but not vascular. On looking at the base of the skull, I observed a spot of the dura mater, corresponding to the roof of the cavity of the tympanum, discoloured, but still retaining its polish, and on endeavouring to raise it, I was rather surprised to find it adhering closely to the bone. On dissecting it away, I found the bone perforated by two small openings, through which a probe entered readily into the cavity of the ear. The friends of the deceased did not allow sufficient time for removing the diseased organ, so that I am unable to state anything about the state of the internal ear.

“It is evident, that in this case a fungus arose from the diseased bone, and that the membrana tympani was destroyed. Upon a former occasion I laid before the Society the particulars of a case in which there was considerable disease of the lining membrane, and otorrhœa to a great extent, but no disease of the bones; yet fatal disease of the brain ensued. This shows that otorrhœa, independent of diseased bone, is quite sufficient of itself to give rise to inflammation and abscess of the brain; but generally some part of the petrous portion is diseased, and we may expect that the locality of the abscess will correspond to the diseased bone. If the antero-superior portion is diseased, the abscess will, most probably, be in the middle lobe; if, on the contrary, on the posterior surface under the tentorium, the cerebellum will be diseased, and it may happen that when the part corresponding to the lateral sinus is diseased, phlebitis may arise, as happened in a remarkable case which Professor Smith brought before the Society many years since.”—*February 11, 1854.*

Ulceration of the Larynx.—Dr. Fleming presented two specimens of disease of the larynx, and gave the following history of them:—The first case was that of Catherine Hand, aged 20, who applied at the Richmond Hospital, complaining of sore throat and considerable dysphagia, so that she could swallow only semi-fluid substances. She was also obliged to measure the quantity, or regurgitation would occur through the nares. She suffered from constant dyspnœa, with distressing cough and expectoration. She had the external features of the strumous diathesis. The whole right arch of the palate was destroyed by ulceration, and the fauces were more vascular than natural. On examining with the finger, only a remnant of the epiglottis could be felt. Ten months ago she caught cold, had sore throat and enlargement of the glands. Five weeks before her admission, she began to suffer from hoarseness and difficulty of swallowing. She never had any syphilitic affection. She was admitted August 3, 1853, not much emaciated, but suffering

from night sweats. The plan of treatment first adopted consisted of tonics and mineral acids, but as no improvement occurred, ten days after her admission mercury, combined with quinine, was ordered. Under this treatment, however, she did not improve. On the 23rd August, she got a sudden attack of dyspnœa, amounting to orthopnœa, with all the symptoms of acute laryngitis, which, however, yielded to treatment. I saw her in the evening, about 10 P.M.; she was then able to lie down, and expressed herself much relieved. The following morning I was surprised to hear that she was dead. She became suddenly worse in the night, and died without difficulty of breathing. During life considerable anxiety had existed relative to the state of her lungs, but we could not conclude satisfactorily as to the presence of tubercles; but the impression on my mind was, that the case was one of tubercular laryngitis, with latent phthisis, and under no circumstances would I have had recourse to operation. On examining the larynx, I found extensive disorganization, with complete destruction of the epiglottis and epiglottidean folds. The lungs were free from tubercle. There was partial suppuration of one of the bronchial glands, most probably owing to tubercular deposit.

The other case was that of a woman, aged 50, admitted September 17, 1853. She had previously been in hospital with anomalous symptoms, leading to the supposition of a syphilitic taint; articular pains, marks of previous ulceration and present ulcers in the throat. Under these circumstances there could be no second opinion as to the nature of her complaint. She got acute laryngitis in an aggravated form on the day after her admission. On the 20th the symptoms were very distressing, and death so nearly impending that it was deemed prudent to have recourse to operation, although the prospect of ultimate success was by no means favourable. She was removed to the operation table, and the first incision only was made, when all present were under the impression that death had ensued. She had ceased to breathe, the pulse could not be felt at the wrist, the countenance was livid, the surface cold; the trachea was opened, but there was no respiration. By means of electricity and artificial respiration, she was resuscitated. She survived the operation upwards of four months, and more than once we entertained a hope of ultimate success. In December she had improved so much that she could breathe freely through the larynx. The wound healed up to a mere fistula. Early in January she got an attack of acute bronchitis, with fetid expectoration, under which she sunk. On examination after death, we found the larynx and trachea ulcerated throughout their whole extent. The lungs did not present any morbid appearances, except those of bronchitis.—Dr. Fleming concluded by making some observations upon the cases in which laryngotomy was proper, and those where it would be better to have recourse to tracheotomy.—*February 18, 1854.*

Cancer of the Uterus.—Dr. Beatty detailed the following case, and exhibited the recent specimen:—A woman, greatly emaciated, was

admitted into the City of Dublin Hospital under Mr. Tufnell, suffering from hemorrhage from the uterus, and was transferred to me. The hemorrhage had recurred on several occasions, with severe lancinating pains in the loins, and down the back of the thighs; on examination it was ascertained that the uterus was in a cancerous condition, the os hard and rugged, and on being exposed with a speculum, well-marked cancerous ulceration was found to exist: at this time hemorrhage was the prominent symptom, and the repeated returns of it were averted by the introduction of fine muslin containing matico leaves moistened with ergot of rye. The disease ran its usual course, the patient becoming gradually weaker, but for the last few months of her life she had no hemorrhage of importance—a circumstance frequently observed in similar cases, the hemorrhage being most severe in the commencement, though the very opposite might be expected, the bleeding vessels becoming more numerous from the extension of the ulceration. Three weeks ago the urine, which had gradually diminished in quantity, became totally suppressed, till the day before yesterday, when she died. Though not expecting to find urine in her bladder, I introduced a catheter, and drew off a few drops of a dark bloody fluid, but no urine. About ten days before her death she was attacked with severe spasmodic pain in the left side of her chest, with great dyspnoea, and a fluttering pulse. The pain was only slightly relieved by treatment, and she sank under well-marked pericarditis. On examining the body we found an explanation of the unusual way in which she died. The ordinary causes of death in this disease are, irritative fever, hemorrhage, exhaustion, peritonitis from the ulceration extending to the peritoneum, or enteritis; but in this case death arose from none of these, but from a cause which I have observed in one or two other cases. There is a preparation in the College of Surgeons Museum similar to that which I now exhibit; and I had lately a lady under my care, who died in the same way. I find no mention of it in any of the authorities, and therefore I think it of importance to dwell on it. The disease commenced in the cervix uteri, and on opening the abdomen we found the uterus not much larger than natural, but excessively hard, and closely attached to the bladder, while on the side there was extensive cancerous development. On opening the bladder it was found that the cancerous degeneration had extended to it from the anterior wall of the cervix uteri; the ulceration was limited to the uterus; none of the usual fungous protusions were found in the vagina. The peculiarity of this case consists in the extension of the ulceration to the mouths of the ureters, so that the renal secretions could not enter the bladder, consequently the ureters became greatly distended; on one side two ureters existed. In consequence of this obstruction the urine entered the circulation; in such cases the patients generally die comatose, except where the extension of the ulcerative process again gives exit to the urine, which then comes away in a deluge. In this case, however, the effusion took place not into the brain, but into

the pericardium, which contained nearly a quart of fluid, dark coloured, thin, and serous. Both surfaces of the pericardium were covered with recently deposited lymph.—*February 25, 1854.*

Rupture of the Chordæ Tendineæ.—Dr. Gordon related the following interesting case of this lesion:—A woman, aged 26, was admitted under my care into the Whitworth Hospital, with violent hemoptysis, and gave the following history of her illness:—she had led a very intemperate life, and had suffered for a long time from palpitation and uneasiness of the heart. Three months before admission she was seized with violent pains in the epigastrium, so severe that she fainted, and did not recover for some time; she always had since then violent palpitations. She had two or three attacks of hemoptysis. She was very pale and blanched, and was labouring under all the symptoms and physical signs of permanent patency; she had plainly visible pulsations in the extremities, and *frémissement* at the root of the neck and over the pericardial region, where there was audible a most intense *bruit de soufflet*; I could not say where this *bruit* was most intense; it was audible over the whole chest anteriorly, and even posteriorly. I stated that I thought it was a case of permanent patency, but from the sudden and violent pain in the region of the heart, followed by intense palpitation and weakness, and from the decided character of the *bruit* and *frémissement* I thought there was a rupture of the chordæ tendineæ of the heart. The woman lived ten days after her admission, and then sank. On examination I found very slight disease in the aortic valves; there was a slight deposit on the central valve; several of the chordæ tendineæ of the anterior portions of the mitral valve were ruptured, and covered with a soft cheesy matter. On taking out the heart it presented somewhat the appearance of a case of well-marked permanent patency, the thickness of the walls being considerable; the apex rounded off, and formed completely by the left ventricle; there seemed to have been slight endocardial inflammation, followed by rupture of the chordæ tendineæ, and this by the effusion of lymph which lay in great quantities loose in the ventricle. The difficulty is to account for the physical signs so closely simulating those of permanent patency. The visible pulsation was very well marked, and the *bruit* was audible in the arteries of the neck. The most probable explanation is that the blood of the left ventricle was allowed to regurgitate through the ruptured valve into the left auricle. The left auricle was greatly dilated. The immediate cause of death was pulmonary apoplexy.—*February 25, 1854.*

Cancer of the Stomach.—Dr. Lees exhibited a specimen taken from the body of a patient who died in the Meath Hospital, into which he had been admitted deeply jaundiced. For the last two years he was subject to dyspepsia. Last July he began to suffer from dysphagia, felt chiefly in swallowing solids, and referred to the xiphoid cartilage. This was followed by vomiting soon after eating; on several occasions the matter vomited was of a dark colour. After

a short time he began to complain of pain in his chest, which gradually extended to the right hypochondriac lumbar regions. In December last he became slightly jaundiced, and gradually more so up to the period of his admission, at which time the skin was of a deep green olive colour. When admitted he was very emaciated, had sickness and vomiting in the mornings before taking food. There was slight enlargement of the liver, but no irregularity of its surface. The alvine discharges were devoid of bile, but the urine was deeply tinged with it. The matter vomited, when examined under the microscope, was found to contain mucus mixed with bile, oil-globules, granular cells, with included nucleated cells, evidently malignant, and blood globules. The urine was deeply tinged with bile, and contained a very dark sediment, consisting of granules, not soluble in a solution of liquor potassæ, and mixed with cells of renal epithelium deeply tinged with bile. On the applications of heat the urine coagulated with a flaky precipitate, not albumen. The biliary matter contained in the urine was composed chiefly of cholic acid, and a small proportion of its colouring matter (or bile pigment). This fact Dr. Lees considered was of importance as bearing on a statement made by Dr. Jones in a late volume of the *Medico-Chirurgical Transactions*, viz., that the dark colour of the urine is merely due to the presence of pigment cells, and not of cholic acid. The jaundice became deeper, and the emaciation increased; the vomiting was checked by bismuth and soda, but he complained of violent rheumatic pains. Dr. Lees considered the case to be one of cancer of the liver, and that the pains were those of cancerous rheumatism.

Post-mortem Examination.—The liver was enlarged, of a deep olive colour, its surface smooth; there were small cancerous depositions in the left lobe; the gall bladder was distended with bile, and the hepatic duct greatly dilated. On the under surface of the liver was a large amount of cancerous deposit; the stomach was adherent to the liver, and its cardiac orifice was surrounded by an enormous cancerous mass, and along its lesser curvature there was a large perforation. The mesenteric glands were in a state of cancerous degeneration. He had no symptoms of pulmonary disease, yet underneath the serous membrane were small white deposits surrounded with vascular areolæ.—*March 11, 1854.*

Pulmonary Concretion.—Mr. Hamilton exhibited a very remarkable specimen of pulmonary concretion, which had been coughed up by a lady, aged 22, whom he had been called to see; he found her spitting up blood, and suffering from intense dyspnœa and severe cough, at times resembling hooping-cough; beside her was a basin containing several ounces of pure blood. At intervals she had returns of the hemoptysis, cough, and dyspnœa, and at last, during one very severe attack, she felt something hard in her mouth, and expectorated the concretion now before the Society. After this she passed a quiet night. During the paroxysm, Mr. Hamilton ex-

amined her chest; it was everywhere clear on percussion, but on the right side, where she complained of deep-seated pain, the respiration was very feeble, as if the air was prevented from entering the lung by some obstruction, as in aneurism. She died of phthisis eighteen months afterwards. The concretion resembled a piece of finely-branched coral, and formed a mould of the smaller bronchial tubes; from its size it would have been deemed impossible that it could have been forced through the rima glottidis.—*March 18, 1854.*

Ulceration of the Trachea and Intestines.—Dr. Gordon exhibited recent specimens of tubercular disease of the trachea and intestines, taken from the body of a man, aged 34, who had been in hospital during the last four months with symptoms of gastric disease; he complained chiefly of a sense of heat in the stomach, and vomiting after meals. He was successfully treated for subacute gastritis; the vomiting ceased, and he then began to complain of cough. On examining the throat the pharynx was found covered with small superficial ulcers; the vomiting soon recurred with well-marked hectic, but at first there were none of the physical signs of phthisis, except that the respiratory murmur was not perfectly clear. His voice was hoarse. He never had hœmoptysis, nor other symptoms of pulmonary disease. He died rather suddenly on Sunday last. On examining the larynx we found the epiglottis ulcerated superficially, and much thickened, and the mucous membrane of the larynx ulcerated deeply. In the trachea there was the usual redness, extending into the bronchial tubes, with numerous small superficial ulcers. The lungs were thickly studded with tubercles. During his illness in the hospital the man had no symptom (with the exception of vomiting) of disease in the abdomen, yet the dissection showed the mucous membrane of the whole intestinal tube crowded with ulcers, except that of the duodenum and rectum. They presented the usual characters of glandular ulcers.—*March 18, 1854.*

Scirrhus of the Pancreas and Liver; Ulceration of the Gall Bladder.—Dr. Lees exhibited specimens taken from the body of a man, aged 58, who, in August last, began to suffer from dyspeptic symptoms: his urine became of a brown colour, he vomited soon after eating, and became gradually jaundiced, and after some time he perceived a swelling in the right hypochondrium.

When he was admitted into the Meath Hospital, he was of a deep olive-brown colour, and greatly emaciated. He complained merely of dyspepsia, flatulence, and a sensation of cold in the lower part of the back. The urine contained cholate of soda. On Friday last he was seized with severe pain in the abdomen; it continued at intervals throughout the night, and in the morning he was in a state of collapse, and the abdomen was much swollen. He referred the pain to the epigastrium at first, but it soon extended over the whole abdomen. He had no vomiting from the time of the first attack till his death, which took place this morning.

On opening the abdomen a large quantity of fetid gas escaped.

The right lobe of the liver was congested, and contained two small cancerous tubercles; the gall bladder was much distended, and perforated on its posterior surface by an ulcer the size of a four-penny piece, thus allowing the bile to pass into the abdominal cavity. The ductus communis was found almost as large as a small intestine; the cystic duct much dilated; and the pancreas forming a large, hard, scirrhus mass, pressing on the common duct at its termination.

Dr. Lees felt uncertain whether the tumour detected during life was caused by the liver being pushed forward by the distended gall bladder (as it felt firm, like an enlarged liver, and not giving a sense of fluctuation), or whether it was owing to the gall bladder itself extending below the umbilicus, and filling up the right hypochondrium. The ulcer of the gall bladder resembled the chronic ulcer of the stomach, the mucous coat being extensively removed, while there was only a very small opening in the peritoneal tunic. There were some clots of blood on the edge of the ulcer; the man had passed blood on several occasions from the bowels.—*March 18, 1854.*

Tubercular Meningitis.—Dr. Lees communicated the following case, and laid the recent specimens before the meeting:—A man, aged 35, was lately admitted into the Meath Hospital, labouring under headach, and having the stupid, oppressed look of a person in the early stage of fever. When I saw him upon the morning after admission, I was told that he had passed a restless night, not raving, but constantly walking about, very cross, apparently out of his mind, and making water about the ward during the night. He would not answer any questions, and would not protrude his tongue, although appearing to be quite conscious of everything around him. He had no heat of head. There was no increased action of the vessels of the neck, nor congestion of the eyes, but the pupils were slightly dilated; pulse 56 and small, action feeble, but both sounds distinct. His head was shaved, purgative medicine administered, and he was put on mercury, &c. Next day his pulse was 42. All this time, though perfectly conscious, he was unwilling to answer questions, or to be disturbed, pulling up the bed-clothes, and catching them in his teeth, but not covering up his eyes as if the light was unpleasant. Upon the fourth day after his admission his pulse was only 45, but upon the eighth day it rose to 118, and for the first time a loud murmur was heard over the heart; there was an evident wavy pulsation in the precordial region. On examination with the stethoscope a loud single murmur was heard, very superficial, and made more distinct by pressure, both at the base and apex of the heart; it appeared to follow the first sound. There was a friction sound in the right pleura. The man died two days afterwards, without coma or convulsions. The urine contained an excess of alkaline phosphates, confirming an observation of Dr. B. Jones, that these phosphates are in excess in cases of cerebral disease. The brain was congested, and the ventricles distended with fluid; the central parts of the brain were diffuent, like thick cream.

The membranes at the base of the brain were vascular, and studded with very minute tubercles, especially about the decussation of the optic nerves. On examining the heart, we found the pericardium vascular in several places, and the surface of the heart studded with granular lymph over both ventricles; the lungs were greatly congested, and the pleura covered with granules, which I consider to be tubercles. The apex of the left lung was studded with miliary tubercles.—*March 25, 1854.*

Dislocation of the Spine.—Dr. Hutton exhibited a well-marked example of this rare form of injury, taken from the body of a man about thirty-five years of age, who, while driving home late on last Saturday night in a taxcart, was thrown into a ditch; he fell upon his head, his neck being doubled under him. He was carried home in a state of insensibility, but next morning he became quite collected; but the lower extremities and the body, as far up as the axillæ, were paralyzed; there was also some impairment of motion and sensibility of the upper extremities; the fingers were flexed and could not be extended, neither could he extend the forearm; he lay flat upon his back and made no complaint, except of pain in the lower part of the neck whenever he was moved. The bladder was distended, the respiration was completely diaphragmatic, and, during each act of inspiration, the capacity of the thorax, although enlarged in the vertical direction, was diminished in its transverse diameter, in consequence of the paralyzed condition of the intercostal muscles. His pulse remained at 62 throughout; his breathing became gradually more embarrassed, from his inability to expectorate the mucus which was collecting in the bronchial tubes. On Tuesday he began to vomit a greenish matter, and died on Wednesday.

Autopsy.—Upon removing the muscles from the vertebral grooves, a space was observed between the fifth and sixth cervical vertebræ, through which the theca vertebralis could be seen; the superior articular processes of the sixth vertebra were also exposed, the fifth having apparently been drawn upwards and forwards, during the forcible and violent flexion of the neck at the time of the accident; the intervertebral substance was torn through, but there was no fracture of the body of either vertebra; the right transverse process of the sixth was fractured, but the lesion had all the appearance of having been caused during the removal of the spinal column. The injury was, beyond any question, a true luxation or diastasis. The theca and posterior vaginal ligament were uninjured, but a small clot of blood existed between them.

The external aspect of the cord was normal, but at the seat of injury it felt soft and pulpy, and when a longitudinal section was made of it, a coagulum was found in its centre, and the medullary tissue was softened around it and discoloured.

Before concluding, Dr. Hutton alluded to the striking resemblance which this specimen bore to that exhibited upon a former occasion by Mr. Adams, in which, besides a fracture of the body of the sixth dorsal vertebra, there was a true luxation between the

fifth and sixth cervical^a. In this instance, also, the cord exhibited externally but little alteration in its appearance from that which is normal, but when a section of it was made, corresponding to the seat of the displacement, ramollissement of the medullary structure was found to have occurred.

Dr. Hutton finally alluded to the cases of the injury in question recorded by Lawrence^b, Dupuytren^c, Samuel Cooper^d, Lasalle^e, Liston^f, Norris^g, and Miller^h, and also to that recently published by Mr. Butcherⁱ.—*March 25, 1854.*

Polypus of the Uterus.—Dr. Beatty exhibited a large uterine polypus, which had been removed by the ligature. The patient, about forty years of age, was a married woman, but never had a child. She was admitted into the City of Dublin Hospital in February last in a state of utter exhaustion from loss of blood, her pulse was weak and fluttering, and her eyelids, feet, and legs were swollen. No examination was made for a few days until her strength was a little restored; she was then placed upon the table, and, upon examination, the vagina was found filled with a large dense tumour, extremely hard, and resembling very much an inverted uterus; it was so large that it could not be included in a four-bladed speculum, and its neck so thick, that the uterus could not be reached. A ligature was passed round it and tightened daily; the ligature broke upon the fifth day, and the instrument came off. Three days afterwards, on proceeding to apply another ligature, Dr. Beatty found that the tumour had come away; the constriction had been sufficient to destroy the vitality of the polypus, though not to cut it through; the sloughing process continuing after the yielding of the ligature had caused the separation of the entire tumour. The woman left the hospital perfectly restored to health.—*March 25, 1854.*

Emphysema.—A remarkable example of this disease was exhibited by Dr. Banks, who gave the following account of the case:—

A man, aged 27, was admitted into the Hardwicke Hospital on the 25th of last month, and at that time laboured under a slight degree of fever. On the third day after admission his respiration became exceedingly hurried and oppressed, so much so that he was unable to lie down, and his face became livid, with an expression of great distress. The most minute examination of the chest failed to detect any abnormal dulness, but dry bronchitic râles were audible over its whole extent. The pulse became extremely rapid, and the respiration was more than proportionately increased in frequency. Over considerable portions of the chest moist râles very soon took the place of the dry, evidently in the smaller tubes. The breathing became more and more oppressed, and on the 5th of this month he died. The symptoms were those of capillary bronchitis. On examination after death the surface of the lungs presented a most beautiful appearance, some-

^a March 29, 1848.

^b Leçons Orales.

^c Gazette Médicale, 1841.

^d American Journal of Medical Science, 1841.

^e Dublin Quarterly Journal, 1853.

^b Medico-Chirurgical Transactions, vol. xv.

^d Surgical Dictionary.

^f Lancet, 1837.

^h Practice of Surgery.

what resembling the spotted skin of a leopard. The lungs were almost universally emphysematous; immediately over the heart there was a small portion of lung completely solid, and in various other situations there were portions similarly altered. The condensed parts were tough, violet-coloured, and their section non-granular,—an example of the state of lung termed lobular pneumonia, atelectasis, *état fatal*, collapse, &c. &c. The anatomical characters of intense bronchitis were present, and a white glairy fluid was found filling the minute ramifications of the tubes. One of the aortic valves was cribriform; water poured into the aorta flowed freely into the ventricle. The specimen illustrates the views put forward by Dr. Gairdner of Edinburgh respecting the genesis of emphysema. The course of events, according to this author, is capillary bronchitis, producing bronchial accumulations and obstruction. Collapse follows the obstruction of the bronchial tubes, and emphysema occurs as its consequence. Dr. Gairdner has reviewed the theories brought forward before his time, and has satisfactorily shown that the blocking up of a bronchial tube, and the consequent retention of air, produces effects the opposite of what Laennec supposed; in this opinion he is supported by the observations of Fuchs, and the experiments of Mendelsohn and Traube, who found that the obstruction caused by a slug impacted in the bronchus of a rabbit produced atrophy of the corresponding lung. According to this distinguished pathologist (Dr. Gairdner)—“It may be safely assumed that emphysema is an *increase in volume* of those portions of the lung to which the air has access, to supply the place of *diminished volume* in those parts from which it is excluded.”

Aneurism at the Base of the Brain.—Dr. Mayne exhibited a specimen of this rare disease, which had proved fatal by rupture of the sac; but as the case (which was accurately observed during its entire progress) will be given in full hereafter, it is now merely placed upon record.—April 8, 1854.

Sanguineous Tumour of the Bones of the Head.—Professor R. W. Smith exhibited the cranium of a man about forty-five years of age, who presented himself at the Richmond Hospital in October last, to have the hemorrhage arrested from an incision that had been made into a tumour upon the head. He had been for some time complaining of pain in the right side, headach, and general dyspeptic symptoms, but he was not aware of the existence of the small tumour upon the head, which was accidentally discovered by the medical gentleman under whose care he had placed himself, and who, having distinctly felt fluctuation in it, and supposing it to be a small abscess, made an opening into it, when nothing but blood flowed out. The hemorrhage was considerable, and having continued for such a length of time as to alarm the patient, he came to the hospital, when the bleeding was arrested by the pressure of a compress and roller. After a few days these were removed, but shortly afterwards the blood gushed out afresh, and was again checked by the same means. When the compress was removed the second

time, the incision was found to have healed perfectly, and nothing remarkable occurred for a few days, but then the tumour became as large as it had originally been, and presented an obscure pulsation, but with a feeling of fluctuation so very decided, that the resident pupil, who had charge of the case, thought himself justified in making a puncture into it; the result was the same as on the occasion of the first opening of the tumour—blood alone was discharged; it gradually, however, ceased to flow, but the wound soon presented an unhealthy aspect: febrile symptoms showed themselves, and erysipelas, commencing about the tumour, spread over the entire of the head, and under this attack the man sunk about a fortnight after the first puncture had been made.

Upon examination *post mortem*, there was found in the right lobe of the liver a large, white, encephaloid tumour; it was not circumscribed, but gradually lost in the surrounding tissue of the organ. All the other viscera, both of the abdomen and thorax, were healthy.

Beneath the tumour of the scalp, in the parietal region, which had been punctured, the bone had been absorbed, and the cranium perforated by an aperture, the circumference of which was as large as that of a sixpence; it was filled by an exceedingly vascular growth, the basis of which seemed to be areolar tissue, the cells of which were filled with blood. In many other parts of the cranium similar perforations, and similarly occupied, existed; some of these vascular growths adhered to the dura mater; the brain was healthy, and no sign of inflammatory action was visible in its membranes. The disease which thus destroyed the osseous tissue evidently originated in the diploe, but it is very difficult to pronounce with certainty as to its exact nature, although the coexistence of an encephaloid tumour in the liver renders the probability very great that the cranial tumours were malignant, and supports the suggestion hazarded by Mr. Stanley, that the sanguineous tumours of bone may be a variety, or the incipient stage of encephaloid disease.—*April 22, 1854.*

Congenital Luxation of the Radius.—Professor R. W. Smith exhibited a striking example of this malformation, taken from the body of a man who died lately in the Richmond Hospital of secondary syphilis, affecting the osseous system extensively. The neck of the radius was so far elongated as to place the head of the bone on a level with the summit of the olecranon process. There was no capitulum to the humerus, nor any vestige of its having ever existed. The head of the radius, surrounded by a fibrous tissue, in the form of a capsule, rested against the outer surface of the humerus; its upper surface was convex, and to its centre was fixed a bundle of fibres, like a round ligament, and derived from the interior of the capsule above alluded to. Professor Smith said he would not now dwell upon the subject of congenital luxation of the head of the radius, having considered it fully at two former meetings of the Society^a.—*April 22, 1854.*

^a January 12, 1850, and December 20, 1851.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

SESSION 1853-4.

(*Continued from p. 246.*)

THIRD MEETING, 31ST OF JANUARY, 1854.

DR. BEATTY made some remarks relative to the administration of chloroform in practical midwifery*.

DR. ATTHILL read a paper upon the exhibition of chloroform in puerperal convulsions. He was aware that this agent had been extensively used by many practitioners in such cases, and, according to some, with universal success; yet he feared that in these cases its power had been overrated, and that while it acted sometimes most beneficially, the experience gained from its administration in the Rotundo Hospital forbade its indiscriminate use, or a too implicit reliance to be placed upon it in eclampsia. Dr. Atthill then detailed the following cases:—

“M. B., aged 18, first pregnancy; without the slightest premonitory symptoms, was seized with convulsions in the second stage of labour. After the first fit chloroform was administered by inhalation. She had, however, two more seizures while under its influence, the only appreciable effect of the drug being the lessening of their severity. The forceps were applied, and the child delivered alive; she was now permitted to recover from the influence of the anæsthetic, but shortly after the expulsion of the placenta, another fit coming on, it was again administered, and with good results, as there was no further attempt at convulsions afterwards. This woman recovered without a bad symptom.”

“E. M., aged 21, first pregnancy; convulsions set in when the head was almost pressing upon the perineum; after the second fit chloroform was given, and in this case, though the fits seemed to have been cut short by the chloroform, yet their recurrence was not prevented. The pains continued strong, and labour was soon terminated by the natural efforts. In the interval she had eight attacks, but they ceased immediately after delivery.”

Here, as well as in the last case, the only effect the chloroform seemed to produce was to render the fits less severe; in both cases they continued till after delivery, and then ceased; in both cases also, no other remedial agent was had recourse to. This woman subsequently became maniacal.

The next case, Dr. Atthill believed, would prove still less satisfactory.

* Dublin Quarterly Journal, vol. xvii. p. 355.

"M. W., aged 36, entered hospital in her second labour; as far as could be learned, her first was perfectly natural. On admission she did not attract any unusual attention, but in a short time began to complain of intense pains in the epigastrium, and sickness of the stomach, which she attributed to the fact of her friends having made her swallow some whisky punch prior to her leaving home: at this time the os was one-half dilated: the labour proceeded slowly till the head was just on the perineum, when she was seized with a convulsion. Chloroform was at once administered, and delivery effected with the forceps; she now proved to have twins, and the second, being also a head presentation, was turned and extracted alive. Chloroform was now discontinued, but after an interval of about twenty minutes, another fit coming on, it was again exhibited, yet without any benefit; she became very restless and unmanageable; a bolus of calomel and jalap was now given, and notwithstanding that the influence of chloroform was still kept up, a third fit succeeded after about an hour's interval. This seizure was the most severe she yet had. The chloroform was again discontinued. On recovery from this last fit, she became more conscious than she had been in the previous intervals, and was able to answer questions; the fits, however, continued to recur every hour, and notwithstanding the other measures had recourse to in connexion with the chloroform, such as bleeding, mercurial purgatives, enemata, &c., she died twenty-four hours after delivery.

"It was subsequently discovered that this patient had two or three slight fits prior to her admission, and it is worthy of remark, that on passing the catheter during the evening prior to her death, the urine, of which there was about half a pint, was mixed with blood, whether from the bladder or kidneys could not be determined, inasmuch as it was impossible to obtain a post-mortem examination."

The following case Dr. Atthill considered not much more encouraging.

"A. Q., aged 19, was brought into hospital at ten o'clock one morning, in a state of complete unconsciousness, having had three fits of convulsions prior to admission; the first stage of labour had not progressed one-third; head presentation; both her upper and lower extremities were œdematous; her urine albuminous, and the rectum greatly loaded. She was at once bled to twenty ounces, her head shaved, five grains of calomel placed on her tongue, and a brisk terebinthinate enema soon after administered. The pains continued to return regularly, and at two o'clock P.M. she gave birth to a living child. Immediately on its birth she had a fit of such severity that it was thought she would have expired in it. On recovery, the inhalation of chloroform was commenced, but it had not the slightest effect in checking the return of the fits, or in lessening their severity. Other treatment was then employed, such as blistering the scalp, cupping the nape of the neck, and subsequently the exhibition of morphia, yet the fits continued to return at shorter intervals, and

she expired forty-four hours after delivery, having had in that period fifty-one fits. On post-mortem examination nothing abnormal could be discovered."

The next case Dr. Atthill noticed was that of M. A. D., aged 30, first pregnancy, still wanting a fortnight of full term; when she applied for admission, it was observed that some œdema of the upper extremities was present; that the lower were enormously swollen; and that the abdominal parietes were also infiltrated with serum; the uterine tumour was very large, and she was so helpless that she could not move without assistance. Purgatives were freely used, but the next day she was seized with convulsions; she was then largely bled, and other measures adopted suitable to her case; after having had fourteen fits, she was put under the full influence of chloroform, and while in this state a grain of morphia was laid upon her tongue; this treatment was productive of most marked benefit, for she fell into a quiet sleep, and the fits entirely ceased; but that this state depended altogether upon the chloroform would be hardly possible to say, since morphia was administered at the same time, and other remedies used prior to its exhibition. During all this time there was no attempt at uterine action, nor did it set in till forty hours after the first convulsions; the labour was easy and natural, and of nine hours' duration; she had no fit subsequent to delivery, but she was at intervals slightly maniacal. On the following day she complained of acute pain in the right side of the chest, and vomited constantly; treatment failed to relieve her, and she died the day after these symptoms set in. The autopsy displayed Bright's disease of the kidneys, and a liver enormously enlarged, soft, and friable, presenting on its surface several large irregular depressions of a bright yellow colour, apparently the result of total disorganization of its structure in these particular regions. Though this case terminated fatally, Dr. Atthill considered that it was in some respects satisfactory, inasmuch as the patient did not die from any puerperal causes, and the treatment in combination with the chloroform was evidently beneficial; but from the fact of morphia having been given along with the chloroform, it was impossible to arrive at a conclusion as to how far the chloroform alone was useful.

In the following case, he thought the most happy effects were due to chloroform by itself.

"E. C., aged 28, strong and plethoric; at admission both upper and lower extremities were œdematous, the former especially so; she was then in labour: first stage two-thirds advanced; parts relaxed and moist; everything went on well till the head became engaged in the pelvis, when she was seized with most violent convulsions; she was at once bled to twenty ounces, after which she became partially conscious, but was soon seized with another fit of greater severity than the former. The head being low in the pelvis, the forceps were at once applied, and the child delivered alive without difficulty; a bolus of calomel and jalap had been previously given, and a purgative enema was subsequently thrown up, but the bowels remained un-

moved; after the lapse of an hour she had a third fit, as severe as the previous one, and they continued to return every hour; the pulse being still full and strong, she was again bled to twenty ounces; two attacks occurred after this second bleeding, making a total of ten fits in eight hours; and with the exception of a few minutes after the first seizure, she remained perfectly unconscious during the intervals. It was now determined to try chloroform, and she was accordingly placed under it, and the influence was maintained for four hours, during all which time she had no return of the convulsions; a full opiate was then administered, and the chloroform was withdrawn; she slept quietly, the next day she was partially rational, but on the fourth after delivery slight symptoms of mania showed themselves, which, however, readily yielded to full opiates."

Dr. Atthill remarked that the convulsions in this case did not yield to bleeding, but the moment that chloroform was administered they ceased entirely. He said that had this case been considered by itself, he might have been induced to look upon chloroform as a certain remedy against this serious complication of labour. It was, however, preceded by five other cases, in two of which the only effect of the anæsthetic was to render the spasms less severe, without hindering their return, and in two others it certainly was productive of no good at all. In these latter the impression on his mind was, that it even rendered the patient more restless, though apparently productive of no actual harm; and that he, therefore, as yet, looked upon chloroform as a doubtful remedy in these kind of cases, and not to be administered indiscriminately; but that without doubt it was suitable to a certain class of convulsion cases, and so the question arose—"How are we to distinguish the cases suitable for chloroform?" or "What are the indications for its use in eclampsia?" and this question, he confessed, he was unable to answer; yet it was important to remember, that in the two successful cases which he had detailed, each patient had been largely bled some time prior to the inhalation of the chloroform. Dr. Atthill thought he had come to the conclusion that chloroform should not be used in true puerperal convulsions until venesection had been performed, unless manifestly forbidden from some other cause. He also considered it worthy of remark, that in the first of the two last-mentioned cases, viz., that in which morphia had been administered along with the chloroform, the patient was in an advanced stage of both renal and hepatic disease,—therefore organic disease of these organs did not necessarily contra-indicate its use.

Dr. Atthill was not prepared to give any explanation as to the *modus operandi* of chloroform in cases of puerperal convulsions, but it seemed possible to him that its action was very similar to that of opium, at least he had seen cases in which morphia in large doses produced like effects, of which the following was an example:—M. H., on her admission into hospital, was quite insensible, having had three fits of convulsions prior to admission. Her legs were œdematous; the rectum loaded with fæces; the pulse 85, hard and

full. She was at once bled to twenty-four ounces, and a bolus of calomel and jalap got down. This was at 2 P.M. on the 14th of April, 1853. Notwithstanding this treatment the fits continued to return every half-hour. At 8 P.M. she was again bled to twelve ounces, and as there was not the slightest effect at dilatation, she was subjected to the douche (tepid). This had the desired effect, for labour set in soon after, 11 P.M. She had sixteen fits prior to the commencement of dilatation. A grain of morphia was now laid upon her tongue, after which she slept a little, and though the pains came on regularly, she had no return of the convulsions till 4 A.M., when the head began to enter the brim. At 7 A.M., it being within reach, the forceps were applied, and the child was extracted alive. From 4 A.M. till 7 A.M., the hour of her delivery, she had six fits, but they now entirely ceased. She gradually regained her senses. On the 17th she was quite rational, and we hoped she would have done well; but on the 18th, symptoms of peritonitis set in, and she died on the 21st.

Dr. Athill said, that in this case,—after bleeding, &c.,—one grain of morphia arrested the convulsions for five hours, and that, had it been possible to have delivered her before the expiration of that time, they probably would not have returned again.

DR. O'REILLY, after some prefatory observations concerning the indiscriminate administration of chloroform in parturition, which he condemned, read a paper upon the local application of the drug, both in the liquid form and in a state of vapour, and illustrated his communication with cases.

The first case to which he drew attention was that of Mrs. D., aged 32, mother of two children. She usually resided in England, where she was last confined under the influence of chloroform. She remained in a state of indisposition for some weeks after, complaining of pain in the right shoulder, extending to the arm and side, of an acute character, which was considered to be rheumatic, and for this disease she was treated. She then came to this country, when Dr. O'Reilly saw her, in consultation with Sir Henry Marsh. It was found that she laboured under chronic pleuritis.

Notwithstanding a variety of treatment was adopted, still the pain in the side continued unabated. Sedative applications, blistering, the administration of narcotics, &c., afforded no relief: the pain yet remained a prominent symptom. Hectic fever and emaciation set in, together with an occasional cough, and mucous expectoration.

By removal to the country, and the use of cod-liver oil, she appeared to improve, but the pain still continued constant. It was now determined to try chloroform, a drachm of which was placed upon a pocket-handkerchief and applied to the side; from this she experienced the greatest ease, and it continued a source of comfort to her for a period of six months, although all treatment availed nothing in arresting the disease.

Dr. O'Reilly said he had employed chloroform in several neuralgic cases in the same manner as above detailed, with success generally, though he confessed in some he found that it failed.

Dr. Hardy's anæsthetic douche having appeared before the profession, Dr. O'Reilly—though sceptical as to the advantage of the vapour of chloroform thus employed, yet knowing that the fluid application of the anæsthetic would be inapplicable in some cases—determined to make use of this instrument on the first opportunity; and one soon presented itself in Mrs. L., aged 45, (twenty years married, and mother of eleven children, the last of whom was five years old,) who came under his care with a recto-vaginal fistula. The parts in the immediate neighbourhood of the opening, and the ulceration itself, were indurated, irregular, and of a cancerous nature. Various treatment was employed, yet nothing seemed to afford relief from pain. Dr. O'Reilly then used a liniment composed of camphorated oil, belladonna, and chloroform, which he directed to be rubbed over the sacrum and hypogastrium, and he also applied the liquid chloroform, as in the former case, yet without any perceptible relief. At length he employed the "anæsthetic douche," which almost instantaneously afforded the greatest comfort; the health and spirits of the patient greatly improved; her powers of locomotion became nearly re-established; and though the fungoid excrescences continued to increase in size, still there was perfect freedom from pain. This lady still perseveres in the use of the douche.

Dr. O'Reilly then alluded to a case of tic douloureux, occurring in a young lady, aged 19, where the use of the anæsthetic vapour douche was most satisfactory, in fact, after continuing its employment for some time, the disease was finally removed. He also drew the attention of the Society to the application of the vapour of chloroform by means of Robinson's breast-glass, with the Indian-rubber exhaustor, in cases where fissures exist on or about the nipple, and gave an instance where this mode of proceeding was productive of great benefit.

He lastly related the history of a case of delirium tremens, where all treatment failed to procure sleep, till he made use of the anæsthetic douche, "by removing the nozzle and puffing the vapour in the patient's face," when he soon became tranquil from fierce delirium, and, in an hour after, sleep set in. The young man recovered.

Dr. O'Reilly stated his views relative to the exhibition of chloroform in every mode; he drew attention to the directions issued by the Academy of Paris for its administration, and concluded by saying, that he thought the profession indebted to Dr. Hardy for his apparatus, which possessed the great advantage of bringing mucous outlets and the surface of ulcers situated either in those passages, or upon the skin, under the action of the vapour of chloroform.

FOURTH MEETING, SATURDAY, MARCH 4TH, 1854.

DR. JAMES BRADY related the following case of scarlatina occurring a few days after delivery.

"A. B., aged 29, of nervous, hysterical habit, was delivered of her sixth child at six o'clock A.M. on the 23rd of November last; labour was short and natural, and the placenta was expelled, with the aid of slight pressure, about half an hour after the completion of the second stage. Everything went on well until the 25th, or sixty hours after delivery; when, as she imagined, owing to undue exposure to cold by neglect of the nurse, she was seized with a violent and prolonged rigor, together with severe pain in the back; there had been some secretion in the breasts during this day (25th). On the 26th it was found that she had passed a wretched night, not having slept at all; she complained of a severe headach, and a burning sensation in the eyeballs; but she suffered chiefly from excruciating pain in the back, which had been constant during the night, demanding the employment of constant pressure, such as is used during labour. This pain, she asserted, was more severe than any she had felt in her confinements; there was also occasional pain in the uterine region, and considerable tenderness over the entire abdomen, increased by pressure; the face was flushed; she could not bear the least light or noise; the skin was dry and burning hot; there was incessant thirst, and a pulse of 120; no sickness of the stomach was complained of; neither was there any soreness of the throat; the lochiæ were suppressed; there was no longer any milk in the breasts; and the bowels were confined. On the 27th the head symptoms were somewhat relieved by a copious epistaxis; the pain in the uterus and the tenderness of the abdomen had diminished after leeching; but in no other respect was her condition materially changed. It was found on the 28th, that so far from any improvement having taken place, her state appeared truly alarming:—she had now scarcely slept for three days and nights; she complained much of weakness and weariness from this want of rest; she expressed great doubts and anxiety as to her recovery; there were present great prostration; occasional delirium; constant thirst; abdominal tenderness and tympanitis; with a pulse of 130, weak and wiry.

29th. Six days after delivery, and third of her illness, a bright scarlet eruption appeared over the entire body, followed by an immediate improvement in all the symptoms, and she obtained several hours' refreshing sleep; her tongue was red and dry, with prominent papillæ; for the first time she admitted to slight soreness of the throat; no ulceration existed, but the tonsils and neighbouring mucous membranes were of a bright red colour.

She was, in every respect, improved on December the 1st. The eruption had entirely disappeared; but the pains in the back and in the uterine region had returned; and at the same time there was

considerable tenderness of the abdomen, which was also full and tympanitic; pulse 120; skin moist; bowels purged.

It was noted on December the 2nd, that the abdominal symptoms had been much relieved by a blister; but for the next four or five days the pain and purging caused annoyance, the former being very severe; at the end of this time, however, these symptoms gradually disappeared, and she slowly but perfectly recovered.

The treatment consisted, at first, in moderate leechings, terebinthinate stupes, and mild doses of mercury and opium; subsequently a blister was applied to the abdomen, and acetate of lead in combination with opium administered.

Perhaps the only point of interest in this case is concerning the diagnosis, and I am anxious to direct attention to this consideration, inasmuch as the particular instance I have detailed illustrates, I think, very forcibly the difficulties often met with on this head in private practice; where, on the first symptoms of serious illness, the practitioner is expected to name at once the disease which gives rise to those symptoms. Now, for the first two or three days there was such a close resemblance to inflammatory puerperal fever, that, I think, few could have positively pronounced her state as any other than such; the violent and prolonged rigor, following exposure to cold, on the third day after delivery, together with considerable pain and tenderness in the abdomen; the high inflammatory fever; the suppression of the lochia and milk,—formed, I think, a group of symptoms that would have strongly justified an arrival at that opinion; there was wanting, however, the peculiar expression of countenance so characteristic of that disease; and the well-marked nervous hysterical habit of the patient of course influenced the diagnosis and treatment.

In a case of recovery from scarlatina, complicated with the puerperal state, recorded by Dr. Henry Kennedy, the disease commenced with the usual symptoms, sore throat and vomiting, and in a few hours the eruption; but in the above case, the most frequent symptoms of scarlatina were absent; there was not the least irritability of the stomach or bowels; and not the slightest soreness of throat until the appearance of the eruption, which did not show itself for three days after the rigor; the severe spinal pains, the violent cerebral excitement, and the state of the tongue, were the only symptoms which could cause that disease to be at all suspected.

It is necessary to mention, that on the morning following the rigor, I visited her after I had seen a case of mild scarlatina for the first time; this person lived in a part of the city remote from my puerperal patient, and I took the usual precautions against carrying the disease; the possibility, though, that I might have communicated infection at once occurred to me, and formed, I need scarcely say, a very unpleasant subject for reflection, which has had the effect of determining me never to visit a case of midwifery on the same day that I have attended one of scarlatina.

In conclusion I may mention, that the only child left in the house took scarlatina, and went regularly through the disease.

DR. M'SWINEY brought forward the following case:—

Mrs. F., residing in the suburbs of Dublin, called to his house on the 3rd of last January, complaining that she had been since the morning of the 1st much troubled with frequent desire to pass water, and great distress in attempting to do so; she at the same time remarked that she was three months pregnant; she was in good health, had eaten and drank as usual, but for upwards of two days she had not passed more than about half a pint of urine, and that with great difficulty and on frequent occasions: she declared that she felt no sensation of distention from retention of urine, and merely desired a prescription to relieve her of the pain she experienced on passing it, as well as the frequency of the calls. Dr. M'Swiney, however, passed a catheter and drew off about seven pints of high-coloured urine, to the great surprise of the patient. Attention was at the time confined only to the investigation of the state of the bladder and the unloading it of its contents, no particular note having been taken as to the condition of the uterus; still Dr. M'Swiney was of opinion that "retroversion" did not exist, neither was his attention drawn to the uterus, in consequence of the ease with which the meatus was discovered, and the facility of the introduction of the catheter, which did not require to be passed to any abnormal length,—circumstances which he considered would hardly have been met with had this displacement existed; moreover, the bladder resumed its healthy state at once, catheterism not being again required; an occurrence not likely to have taken place had there been an unreduced retroversion.

As this patient had slight pains on the morning of her visit, accompanied by a trivial "show," with a certain sense of general pelvic uneasiness, she was warned that she might abort, and certain routine directions were enjoined. As predicted, on the morning of the 5th she did miscarry, without the occurrence of anything worthy of detail. Dr. M'Swiney was not with her at the time of her miscarriage, but he saw her some hours after; he was then "shown the foetus without the placenta," and on making inquiry upon the subject, he was assured by the patient herself "that all was right;" that the after-birth had certainly "come away," and that if it was not to be seen, it must have been thrown out.

Dr. M'Swiney regretted to say that he was content with this assurance, made by one who had been confined four times previously to her present miscarriage, and he left her without having satisfied himself as to the correctness of her statement. For the four following days she went on well enough, but on the fifth (January the 9th), he was hastily summoned, early in the morning, to her bedside, when he found her, as he believed, moribund: he learned that she had been "flooding" for some hours, and had lost an enormous amount of blood. He got her to swallow a draught containing a

drachm of laudanum, and administered appropriate stimulants. As soon as possible a vaginal examination was made, when "the placenta" was found to be partly through the os, by which it was tightly grasped; two fingers only could be introduced, and it was removed piecemeal. During this manipulation Dr. M'Swiney had frequently to stop for the purpose of giving his patient some stimulant, as she more than once appeared to be dying. Dr. M'Swiney regretted he had not with him the tenaculum recently exhibited to the Society by Dr. Churchill, as he considered it would have been admirably suited for this particular case. This patient rallied after an anxious period of uncertainty, and she is now comparatively well, suffering merely from the remains of anemia.

Dr. M'Swiney considered this to have been a case in which there occurred, in the first instance, a spasm of the urethra, induced (according to the history given), by the influence of severe cold upon a person of a highly nervous temperament; and subsequent paralysis of the bladder from over-distention; that this largely distended bladder made pressure on the uterus, which, though insufficient to produce retroversion, was yet great enough to cause such an amount of disturbance in that organ as "to stimulate it to throw out its contents." He had already given his reasons for supposing that retroversion did not exist; yet as some might consider this an open question, he not having especially examined for that displacement, he deemed it necessary to state that at no period whatever was there any sign of pressure on the rectum; a dose of castor-oil taken the morning of the day he first saw her, acted without difficulty, and she never complained of any want of power in evacuating the contents of the bowels.

Dr. ATTHILL wished to lay before the Society the statistics of those cases in which chloroform had been administered in the Rotunda Hospital, from October, 1851, up to the present date; but before doing so, he was anxious to explain briefly the class of cases in which it had been administered, as thereby the results would be better estimated.

Dr. Atthill said, "the Master of the Hospital did not consider it necessary or expedient to give chloroform in the ordinary run of natural labours. Its use in the institution was confined—1st. To all cases in which it became necessary to use instruments. 2nd. To all cases where version was contemplated. 3rd. To a few cases of preterm natural presentations of the breech or foot. 4th. Occasionally in a few instances of protracted labour, where the patient was exhausted from long-continued suffering. 5th. In some cases where the woman seemed to suffer more than ordinary, and was nervous, irritable, or noisy. And it had also been tried in a few cases of convulsions, with the particulars of which the Society has been already made acquainted.

In the first or instrumental group, the value of chloroform can be scarcely overrated: the patient exhausted, perhaps, by a tedious

labour, is restless and impatient, and the idea of an instrumental delivery may have added terror to her already unhappy condition; the forceps, consequent on the above state of necessity, or because of restlessness, may not be applied with facility. Now the anæsthetic influence of chloroform obviates all this. The patient can be easily placed in any convenient position, and the comfort experienced by the operator is only equalled by the fact of the patient's own freedom from pain, and the consolation, on her recovery from the effects of the drug, at finding all cause of anxiety and distress at an end. Chloroform is never omitted in these cases.

In the next class of cases, the beneficial effects of the anæsthetic is equally great; the patient is rendered passive, and version is more easily accomplished from the uterine action being suspended (which is the case as a general rule) for the first few minutes after the full effect of chloroform has become apparent.

In the third set, viz., those instances of breech or footling presentations, in which difficulty may be expected in the extraction of the head, from the impossibility of keeping the patient quiet; especially when such presentations occur in primipara, it has been found useful to administer chloroform when the breech begins to distend the perineum; this practice has been attended with the happiest results.

With regard to the fourth class, it has been found, on several occasions, that when the patient was getting worn out from continued suffering in a labour rather tedious, the most marked benefit followed the exhibition of chloroform; for after having been kept under its influence for a time, perhaps for an hour or two, the woman has awakened much refreshed, uterine action has set in with vigour, and the case has soon terminated happily and naturally.

An example illustrating this remark occurred but a few days since in a primipara, in whose case the membranes having ruptured at the very onset of the first stage, tediousness was the result, and when the os was fully dilated, the pains, though incessant and harassing, were so inefficient that they had not power enough to force the head into the brim; the woman, who had, up to this time, been twenty-six hours in labour, and was much exhausted, was now placed under the influence of chloroform, and its effects kept up for an hour at least. After having slept some time, she awoke refreshed; the pains set in powerfully, the head soon rested on the perineum, and the labour was then cut short by the forceps. I do not think that the head, in this instance, would have entered the pelvis at all, had not the woman been refreshed by the interval of ease which she enjoyed while in a state of anæsthesia.

With respect to the peculiar effects produced by chloroform, when used to complete anæsthesia upon the labour patient, the experience of the Hospital confirms the observations made by Dr. Denham in his paper on this subject, published in a former number of the Dublin Quarterly Journal, viz., "that uterine action is at first suspended, but that it usually sets in again after an interval

of about a quarter of an hour; that the pains return regularly and forcibly, and that the actual expelling power of the uterus is not much, if at all, diminished." I consider, however, there is, without doubt, a greater tendency to postpartal hemorrhage in such cases than in those where chloroform has not been inhaled; also, that the uterus does not contract so rapidly or so firmly as usual; and that there is a decided tendency to relaxation in the organ. This, I think, admits of an easy explanation, from the fact that, although the "active contractions" of the uterus are not interfered with, the "tonic action" is more or less destroyed, and hence the tendency to flooding; but this tendency has never resulted in anything serious in the hospital practice, due precautions being always taken. Pressure has constantly to be kept up for a considerable time; sometimes it has been necessary to apply cold perseveringly, and as a general rule the binder is not applied till the woman has completely recovered from the effects of the drug.

None of those unpleasant symptoms, which have been ascribed to the use of chloroform, have come under the notice of those connected with the Lying-in Hospital. The pulse has never once faltered in any of the cases I am about to lay before you. In one solitary instance the patient showed symptoms of hysterics after having inhaled a little, and the inhalation was at once discontinued. And, in another case, when just recovering from its influence, the woman had two hysterical fits, but so short and trivial as to be hardly deserving of notice; with, then, these two exceptions (if they can be called exceptions), all these cases, in which chloroform was administered, were without any drawback, so far as the chloroform itself was concerned."

The following are the statistics which Dr. Atthill presented to the Society.

Cases in which chloroform has been administered in the Rotundo Hospital from October, 1851, up to the present date.

No. 1.—FORCEPS CASES.

Total.	CHILDREN.			MOTHERS.			CAUSES OF DEATH TO MOTHERS.				RATE OF MORTALITY.
	Alive.	Dead.	Dead and Putrid.	Recovered.	Died.	Maniacal.	Peritonitis.	Rupture of Uterus.	Convulsions.	Phlebitis.	
82	69	15	2	78	4	1 ^a	1	1	1	1	1 in 20½

^a Subsequently recovered.

No. 2.—CROTCHET CASES.

Total, including one Evisceration.	MOTHERS.			CAUSES OF DEATH.				RATE OF MORTALITY.
	Recovered.	Died.	Maniacal.	Peritonitis.	Convulsions.	Rupture of Uterus.	Sloughing.	
38	30	8	1 ^a	2	1	3	2 ^b	1 in 4 $\frac{8}{9}$

No. 3.—VERSION CASES.

No. 4.—FOOTLING AND BREECH CASES.

Total.	CHILDREN.			MOTHERS.		CAUSES OF DEATH.		
	Alive.	Dead.	Dead and Putrid.	Recovered.	Died.	Peritonitis.	Rupture of Uterus.	Convulsions.
13	9	4	0	12	1	0	1	0

Total.	CHILDREN.		MOTHERS.	
	Alive.	Dead.	Recovered.	Died.
3	3	0	3	0

No. 5.—NATURAL CASES.

No. 6.—CONVULSION CASES DELIVERED NATURALLY.

Total.	MOTHERS.		CHILDREN.	
	Recovered.	Died of Peritonitis.	Alive.	Dead.
4	3	1 ^c	3	1

Total.	CHILDREN.			MOTHERS.		CAUSES OF DEATH.
	Alive.	Dead.	Dead and Putrid.	Recovered.	Died.	
2	1	0	1	0	2	2

No. 7.—PROLAPSE OF FUNIS DELIVERED NATURALLY.

No. 8.—ALL CASES IN WHICH CHLOROFORM WAS GIVEN.

Total.	MOTHERS.		CHILDREN.	
	Recovered.	Died.	Alive.	Dead.
1	1	0	0	1

Total.	MOTHERS.			CHILDREN.		
	Recovered.	Died.	Total, deducting Perforation Cases.	Alive.	Dead.	Dead and Putrid.
143	127 ^d	16	110	85	21	3

^a Subsequently recovered.

^b One an old vesico-vaginal fistula.

^c Very protracted in first stage, and slow in second. Child dead-born.

^d Including the two maniacal cases, which subsequently recovered.

"Out of this list of 143 cases in which chloroform was administered, there were but 16 deaths; a small proportion, when it is remembered that, with the exception of 7, all were cases of either difficult or complex labour; deducting these from the 16 fatal cases here recorded, 4 of convulsions, in which chloroform was given as a last resource, and which would have undoubtedly died under any circumstances, we have 12 to dispose of: 4 of these died of peritonitis, of which four, 2 were so deformed as to have given extreme difficulty in delivery; 5 were cases of ruptured uterus; 2 were destroyed by sloughing of the soft parts, and one of these was the subject of an old vesico-vaginal fistula, in whom the constant dribbling of the urine after delivery excited inflammation, which, taking on an erysipeloid form, terminated in the destruction of all the soft parts in the neighbourhood of the outlet as well as those of the passages; lastly, in 1, life was terminated by phlebitis.

"I therefore conclude that, on reviewing the causes of death in such of these anæsthetic cases as terminated fatally, no blame whatsoever can be attached to the chloroform. In the paper by Dr. Denham, before alluded to, he gives two instances where anæsthesia could not be induced; with us chloroform never failed to induce this state, save in the one instance, when its use was discontinued; but all the conclusions to which he has there arrived have been fully borne out by this additional experience; in one point only do I differ from him, and that I have already mentioned, viz.: I feel forced to consider that in labour cases, when chloroform is used, there is always a great tendency to post-portal hemorrhage."

Case of Tumour of the Right Labium Pudendi. By FRANCIS CRUMPE, M.D., Surgeon to the Co. Kerry Infirmary.

THE following case, taken from my note-book, may be worth publishing just now, when so much mention is made about fibrous tumours growing from the labia pudendi.

Joan Brien, aged 32, a robust, healthy woman, the wife of a respectable farmer, and in the sixth month of her pregnancy, called on me to remove a tumour growing from the inside of the right labium pudendi: it was remarkably firm and heavy, smooth and glossy on its surface, and in the erect position extended to the middle of her thighs. It was covered by the thin mucous membrane of the labia, which, from constant exposure to the air, had lost its natural moist villous appearance. The fold of skin forming the external side of the labium formed no part of its coverings. The circumference of the tumour transversely was eighteen inches, and the circumference in the longitudinal direction was twenty-two inches. From resting at its base on any surface on which she sat, which, for convenience to herself, was often the ground, it was there excoriated. This woman stated that she was fourteen years married, and had

seven children; that at the delivery of her first child, this labium sustained some violence and laceration; that shortly after a thickening became observable to her in that part, and gradually increased to the size of an orange; that it remained stationary at this size for some years, and did not in any way interfere with her succeeding accouchements; that in the early period of her present pregnancy it began and continued to increase to its present enormous magnitude. On drawing the tumour downwards, and grasping between the fore-finger and thumb its attachment at its root to the inner surface of the labium, it presented the feel which is communicated in the examination of varicocele of the spermatic cord, and the vessels here were as large. The woman was laid on the table as in lithotomy; an assistant standing on her right side was directed to introduce the fore-finger of the left hand as high under the arch of the pubis as he could in the superior commissure of the vagina, and then strongly to compress the vessels, which if he did not succeed in effecting, he was to press the fore-finger of the right hand at the same time upwards in the external fold of the labia, so as to bring the fingers into as near contact as he could: making by two sweeps of the scalpel a semi-elliptical incision on both sides of the mucous fold, commencing at the upper margin of the tumour, it was at once detached. Two arteries, fully as large as the radial, were secured; one suture in the centre of the wound, compresses, and a T-bandage, were applied. The wound united partly by adhesion, and partly by suppuration, and was perfectly healed in fourteen days. On examining the tumour after its removal, one artery, as large as the radial, traversed it in a continuous trunk from the summit to the base. The covering admitted of no mobility, and from its thinness and strong adhesion could not be removed by the scalpel from the tumour, which was void of cellular tissue and fat. In making a section of it, both in the transverse and longitudinal diameter, it presented all the appearance which is seen in the udder of the cow, and not unlike the structure observable in the female breast; it might be described as a variety of the mammary sarcoma.

Case of Chronic Peritonitis, with an enormous amount of Fat in the Effusion; Fatty Degeneration of the Liver; Free Fat in the Blood; Fatty contents of both Ovaries combined with Hair and Bony Matter. By JOHN POPHAM, A. B., M. D., Physician to the Cork North Infirmary, &c.^a

THE pathology of fatty diseases has been as yet so little elucidated, that it is desirable to place on record any remarkable examples in which these deviations from the healthy state of the system occur. I do not recollect any published case where an amount of fat, as in the present instance, was found in a dropsical effusion.

Johanna Murphy, aged 28, unmarried, a servant by occupa-

^a Read before the Cork Medical Society.

tion, residing at Blarney, was admitted to the North Infirmary, under my care, on February 4, 1853, with abdominal dropsy. She exhibited the appearance of well-marked cachexia, being greatly emaciated, the enormous distention of the abdomen strongly contrasting with the approximated ribs and contracted dimensions of the thorax. The history which she gave of herself was, that she had enjoyed good health up to last harvest, when she was attacked with influenza, and afterwards with fever, for which she was admitted into the Fever Hospital. After leaving the hospital, she got dysentery, her health began to decline, and a visible swelling of the abdomen commenced, attended with considerable pain in the hypogastric region. This gradually increased, and the legs subsequently became œdematous; when admitted, the abdominal walls were so greatly distended, that it was impossible to distinguish the precise nature of the contents. She had no sign of jaundice, or pain in the hypochondrium; no tenderness on pressure; the bowels were confined; no nausea or vomiting existed; pulse 96, weak; great oppression of the respiration; urine scanty. The catamenia had appeared at the age of 15, but were subsequently irregular; they ceased at the period of the fever, and did not return. There was little hope of any benefit from medical treatment; and the dyspnœa increased so much, that at her urgent request paracentesis was performed on the 23rd of the month, and twenty-five pints of a thick, oily fluid, of the colour of porter, were drawn off, after some curdy matter had first passed. She received little relief from the operation, and died three days after of exhaustion.

The body was examined by the house surgeon, Dr. Donegan, twenty-four hours after death. Great emaciation existed of the upper, and œdema of the lower extremities. On making a section of the abdominal parietes, the first thing that struck us, after the escape of a quantity of thick, turbid liquid, was a *cake of solid fat*, like tallow, floating on the surface. It was flat and smooth, about the size and form of a small plate, and nearly half an inch in thickness. In consistence it was rather soft, like butter, and not unlike it in colour. It closely resembled the fatty cake which collects on the surface of broth on being cooled. Besides this concrete mass, there were numerous flakes of soft fat, from the size of a grain of shot to a pea, mixed up with the fluid, which was of a dark colour and oily appearance. The amount of fluid had pushed up the diaphragm so considerably, that, looking from below, there appeared almost a total absence of the thoracic cavity. Upon removing the fluid and washing out the peritoneal cavity with care, none of the abdominal viscera were in sight, being concealed from view by a dense layer, seemingly of lymph (for it presented none of the characters of omentum), of a quarter of an inch in thickness; this following the course of the peritoneum, on the back of the abdominal wall, passed along under the surface of the diaphragm, and then continued down, over the liver, stomach, and intestines, into the pelvic cavity, so as to incarcerate the abdominal and pelvic viscera, and to insulate the drop-

sical effusion, thereby rendering it of an encysted character. The free surface of this layer was dark like soot, being so thickly spotted with a melanotic deposit as to present a continuous black appearance. Projecting from the general outline was an ovoid tumour, rising from the pelvic region, and swelling out into the hypogastrium, which proved to be the right ovary. On removing the investing layer, the intestines were found compressed and agglutinated together by short bands of lymph mingled with the same yellow flakes of fat. The liver was a little over the natural size, soft, of a yellowish-brown colour, and in an advanced degree of fatty degeneration; the hepatic cells were distended by oil globules; the gall-bladder contained a small quantity of tarry bile; the ductus communis choledochus was not obstructed. No disease was found in either pancreas or spleen. The kidneys were healthy, but the ureters were dilated at their renal terminations, especially the right; the bladder was closely constricted. In the substance of the mesentery several small fatty tumours were found, containing a substance like adipocere, the largest about the size of an almond; they consisted of an unilocular sac, with very thin walls. The blood in the inferior vena cava was loaded with oil globules. The mucous membrane of the stomach was of a dusky colour, and a similar leaden hue was seen along the intestinal canal.

The lungs were so much compressed as to seem almost atrophied; the heart was normal; the lining membrane of the aorta was of a bright vermillion colour; no tubercles existed in the lungs.

The head was not examined.

The uterus was rather less than the natural size, the os tincæ very small and round, giving to the finger the sensation which French writers compare to that felt by touching *le bout du nez*; the vagina was very narrow, scarcely admitting the finger; the hymen was unbroken, so that all these evidences rendered it a matter of certainty that the products found in the ovaries were not caused by the stimulus of a Graafian vesicle from intercourse.

On making an incision into the pelvic tumour, and which was the right ovary enlarged nearly to the size of an infant's head, a quantity of hair was disclosed in an unilocular cyst, closely matted together and forming a ball about the size of a goose egg. This hair was intermixed with a melicerous deposit, inodorous; it was not attached to the walls of the cyst, but lay perfectly loose. Its colour was a deep brown, most of it but a few inches in length, but some more than a foot; it was fine and silky. In some, the hair bulbs existed as in normal hair; while in others no appearance of them was seen. It was most probably, as Vogel observes, originally developed from hair sacs implanted in the cyst, but afterwards detached by some unexplained cause and detained in the ovary from the property of this substance to resist absorption. The fatty matter was mixed up with it in great abundance, and resembled honey partially candied; it was most likely, as Cruveilhier supposes, the product of the secretion of the sebaceous glands which accompany

the hairs. The left ovary was about the size of an orange, and had similar contents, less hair, but more of the melicerous deposit. It formed an exception to the general rule laid down by Rokitanski, who states that it is rare for *both* ovaries to be affected with those adipose cysts. In the walls of each ovary, bony masses existed, like knots projecting from their surface; and on cutting down on them, portions of true bone were found, in some, of an amorphous character, but having a periosteum, while others were teeth-sacs containing teeth, chiefly incisors; some of the latter were single, in one case, two were blended together. Some of them had fangs infixed in bony alveoli.

In the preceding case several remarkable peculiarities existed, so that it becomes difficult to theorize with certainty upon the rationale of the disease. Thus we have fatty degeneration of the liver, fat in the blood, peritonitis with an effusion of a remarkably fatty character, and, lastly, ovarian disorganization, in which fat forms a prominent element. If we look to the history of the case for a clue to guide us, there seems no ground to believe that the morbid changes discovered after death were in existence antecedent to the fever, so that the whole circle of pathological results would seem confined to a period of six months or less. The typhus fever occurring during the preceding autumn was regarded by her as the first breach of health; but it is difficult to ascertain in what part the structural changes commenced. We might conjecture, indeed, that in the altered nutrition of the body which takes place in fever, the fat was taken up by the veins from its ordinary locality, and was deposited from the blood in the hepatic cells, which possess a peculiar attraction for this substance; or, that the changes in the intestinal tract produced by typhus may have prevented the saponification of fatty matter, and thus allowed its retention in the blood. Perhaps the morbid state of the intestines in phthisis may have some influence in determining the fatty degeneration of the liver, so often found in that disease: the same state of liver has been found by Dr. Bright to coexist with chronic dysentery. The typhus infiltration itself has been proved to be of a lardaceous nature, and a similar deposit is found in the mesentery. We have not certainly on record cases of fatty degeneration of the liver as the result of continued fever, though Rokitanski has noticed this state in connexion with ague. If we look to the causes of fatty degeneration of the liver, assigned by the same author, none of them apply to the present case: these are tubercles, excess of rich, oily food, and abuse of alcoholic liquors; but these causes act differently; tubercles, by lessening the capacity of the lungs for the oxidation of carbonaceous matters in the blood; the latter causes, by introducing these matters in undue amount into the circulation. Certainly both severe pulmonary and intestinal diseases are associated with an augmented state of fat in the blood. Simon found an excess of free fat in the blood in pneumonia.

From the above case it would appear that an unhealthy state of

the pancreatic fluid is not necessarily connected with an over-amount of undissolved fat remaining in the system, as this organ was free from lesion. The excess of this element was probably due in part, at least, to a defect in the assimilating powers of the liver, a sufficient quantity of bile not being separated to dissolve the fat, so that an undue amount of the adipose element remained in the blood. It is difficult to explain the deviation of its direction from normal to abnormal parts,—why, for instance, it should be studiously averted from parts where it is a natural element to places where it becomes a foreign and mischievous body. It is thus we find it in the ovarian cells, perhaps, from their misplaced activity upon excitation by the stimulus of fat in the blood, especially consequent on the arrest of the catamenia, one of the sources by which excess of carbon is removed from the female system. Finally, are we to regard the peritonitis as the last link of the chain of disorder, and a closing effort of nature to free that fluid by exudation of its abnormal product; or as a secondary disease spreading from the disorganized ovaries by contiguity of peritoneal tissue, something in the way that puerperal peritonitis is associated with inflammation of the uterus after parturition?

In the present state of our knowledge, these matters are difficult of explanation; and it is only by experiment and careful induction they can be answered. As yet, we have no data sufficiently conclusive.

A Case of Labour rendered difficult and protracted by adhesion of the Placenta, and by the Funis having been coiled three times round the Child's Neck. By JOHN KEARNS, M.R.C.S. England, Surgeon to the Kilkenny Dispensary.

ON the night of Thursday, the 20th of October, 1853, I was sent for to see Ellen Talbot, forty-two years of age, the wife of a small farmer, in labour of her first child. She stated to me that, according to her own calculation, she wanted about a fortnight of her full time, and the midwife informed me that she was forty-eight hours in labour, the waters having escaped twenty-fours before my visit. On examination I found the os uteri dilated to the size of a crown-piece; the membranes not completely broken, so that when the pains, which were good, occurred, some liquor amnii presented before the child's head. Her pains being strong, with a pulse of 90, I left her to the natural efforts till the following day. On Friday, the 21st, I found the liquor amnii entirely evacuated, the os uteri more than two inches in diameter, the pains good, her pulse 90, and firm; but I found that the pressure of the head against the os was not at all equal to the uterine action during a pain, and that it was rather strongly retracted in the interval. However, considering interference uncalled for, I saw her again in the evening, when I found the os uteri still more dilated, with the head pressing more strongly against it. To contribute to the further dilatation of the os uteri,

and to procure for my patient an interval of repose in so long and unremitting a labour, I gave her an opiate and left her in the nurse's charge for the night. I saw her again early on Saturday morning, and found her pains strong and frequent, the head of the child having cleared the os uteri, or rather the latter having passed behind the former, as the head had made little or no progress. After a couple of hours I found that, although there was no pelvic obstruction, there was no advance of the child's head; that it did not press against the surrounding parts as strongly as the uterine action was vigorous, and that it receded quickly as the pains subsided.

Having failed in procuring additional medical aid, I determined on using the forceps for the purpose of bringing the head slowly down on the perineum, and having applied the instrument with care, by gentle traction during the pains the head was at length placed on the perineum.

I now left the remainder to be accomplished by the natural efforts, and although the pains were still as vigorous as ever, with a perineum not sufficiently rigid to account for it, the head of the child was not delivered for twelve hours after the use of the forceps.

On the birth of the head, I found the funis coiled three times round the neck of the child, the integuments of the head and face being quite livid. I tied and divided the cord, when the remainder of the birth was quickly completed by the natural efforts.

Having now waited an hour for the delivery of the placenta, and there being no sign of its expulsion, the uterus being, at the same time firm, and no hemorrhage taking place, I thought it better to let my patient rest than to interfere for the present. I, therefore, left her bandaged with a napkin applied in the usual way, and with strict directions to the nurse not to remove her hand from over the uterus till my return. In five hours after she was just as I left her, and having introduced my hand for the purpose of removing the placenta, I found the greater part of it in the vagina much lacerated; the remainder firmly grasped within the uterus. By a patient dilatation of the os uteri, and by a careful peeling off, I succeeded in removing the very strongly adherent portion.

Although the age at which this woman became primiparous would dispose one to consider that rigidity had much to do in the case, yet I am satisfied that the difficulty and delay were entirely due to the strongly adherent and rigid placenta, and to the funis being thrice coiled round the child's neck, thereby shortening the cord considerably, and tightly attaching the infant to the fundus uteri or to its immediate neighbourhood.

The occurrence of so many coils round the neck may be accounted for by the occupation of the mother, who was daily in the habit of raising a heavy milk-pail to her head from her knee, aided by a sudden jerk of the latter, thereby tilting up the child's head on these occasions. The occurrence of the third coil was probably the cause of labour setting in before the term of gestation had been fully completed; and it is likely that it was by the same accident

and at the same time that the infant was strangled, from the livid and partly decomposed appearance which it presented, together with the offensive odour with which the birth of both fetus and secundines was accompanied.

The strongly adherent condition of the placenta may be owing to inflammation and deposition of lymph, caused by the irritation and dragging to which it must have been repeatedly exposed.

For the first three days after delivery, my patient was doing tolerably well. On the fourth day fever of a low kind set in, and on the sixth day an attack of serous diarrhœa, the most sudden and profuse that I have ever witnessed, came on, and she died on the seventh day.

Observations on Urine and Urinary Calculi. By WILLIAM
D. MOORE, A.B., M.B.

1. *Urinary Calculi, consisting of Heller's Urostealith.*—On the 27th July, 1853, I received by post, from Dr. Little, of Sligo, two very small, dark-brown calculi, with a request that I would subject them to chemical examination. Of these concretions I sent Dr. Little the following account:—

The calculi, when bruised in a mortar, are found to be soft and of a peculiar waxy consistence; triturated with water, they, with difficulty, divide into small portions, having a decidedly waxy appearance. In water of caustic potash they are immediately broken up, and become white and soapy-looking; but by far the greater portion remains undissolved. Dilute nitric acid seems to have very little action on them. Heated in the flame of a spirit-lamp they fuse, and afterwards burn with a bright flame, leaving a blackish ash, this before the blow-pipe becomes brilliantly incandescent and leaves a beautifully white, highly alkaline residuum, evidently caustic lime. A considerable portion of the calculus is dissolved by boiling alcohol, and separates on partial evaporation and cooling, as a whitish deposit. In this deposit microscopic examination exhibits numerous fat corpuscles, but no crystalline plates. The calculus would, therefore, appear to consist of oxalate of lime closely united to a peculiar fatty matter, which is not cholesterine.

Such was the impression an examination of these calculi made on me before I was aware that the substance of which they were composed had been previously described. The frequent occurrence of oxalate of lime in urinary calculi and the similarity of its behaviour before the blow-pipe to what I have described, led me to conclude that the lime I obtained from these concretions by the action of heat had existed in them in the form of oxalate. In this, as will hereafter appear, I was mistaken.

On the 13th of March, 1854, I received from Dr. Robert Adams two calculi taken from the body of the patient by whom both of the above-mentioned specimens had been passed in the urine. One was large, being the size and nearly the shape of a small hen-egg; in the

centre was a cavity containing, but not filled by, a quantity of the dark-brown substance already described. The layers of the calculus next adjoining this substance were white and friable, and before the blow-pipe instantly melted into a white pearly globule. In water of caustic potash they were insoluble, and gave off ammonia; they readily dissolved in dilute nitric acid. This portion evidently consisted of the so-called fusible calculus, a mixture of the ammoniaco-magnesian phosphate with phosphate of lime. The most external coat, which was very thin, was much harder and was of a light-brown colour; it appeared to be composed almost entirely of phosphate of lime, dissolving readily in dilute nitric acid, but not in cold acetic acid, and remaining unfused when exposed even to a considerable heat before the blow-pipe. The residuum left after the action of the blow-pipe was very faintly alkaline.

The second calculus given me by Dr. Adams was very small, and appeared to be formed of phosphate of lime.

Never having before met with a fatty matter existing in so large quantity as a constituent of urinary calculi, I now proceeded to ascertain whether a description of any such case was on record, and soon lighted on the account of Heller's Urostealith, given in Simon's *Animal Chemistry*^a. With this substance I am convinced the nucleus of the larger calculus is identical. To the description of the properties of this body there given I would only add the appearance of oil globules presented under the microscope in the alcoholic solution when cooled or partly evaporated, which is important as diagnostic between the urostealith and cholesterine. I would also observe that the fragrant odour mentioned by Heller as being given off by the urostealith when heated, and compared by him to that of shell-lac and benzoin, seems to me to resemble closely the smell of musk, or, more exactly, the odour emitted by burning cascarilla bark. Lastly, it is more likely that the lime which remained after the action of the blow-pipe had existed in combination with the peculiar fatty matter, than that it was present in the nucleus as oxalate of lime. Heller's observation, as quoted by Dr. Day, in the edition of Simon's *Chemistry* I have referred to is, that after the combustion of the urostealith, "a voluminous coal is left, which, when thoroughly burned, leaves a very minute alkaline ash, consisting principally of lime."

Being anxious to decide this point, I procured a fresh portion of the nucleus, which Dr. E. W. Davy was good enough to examine for me. Dr. Davy's experiments confirmed the view I had taken of the close agreement of the characters of this substance with those of Heller's urostealith. As to the state in which the lime exists, he found part to occur as the phosphate, and the rest "to be in combination with the fat or waxy-substance, forming some organic combinations with the fatty acids." In all his experiments he failed to get any unequivocal evidence of the existence of oxalic acid. It is,

^a Sydenham Society's Edition, vol. ii. pp. 326 and 452.

therefore, more than probable that none of the lime was present in the form of oxalate^a.

The nucleus then of the large calculus, furnished to me by Dr. Adams, consisted of urostealith, the middle layers were composed of the combination of phosphates which has received the denomination of "fusible calculus," while the outermost coat was formed of phosphate of lime. The identity of the nature of the nucleus with that of the calculi sent to me last summer by Dr. Little is worthy of note, while the demonstration of the urostealith is, from its extreme rarity, which is proved by its not once occurring in the long list of calculi given in Simon's Chemistry^b, nor in the enumeration by Dr. Golding Bird of the calculi in Guy's Hospital Museum, and in the Transylvania University Museum, of which sections have been made^c, highly interesting. I may add that the present case exhibits this substance under the two forms in which it occurred to Heller, viz., "either pure, or having an outer coating of ammoniaco-magnesian phosphate." In the instance which came under his observation it was presumed, in consequence of the locality of the pain, that the calculi were renal.

2. *On an unusual Combination of Properties in a Specimen of Urine.*—It is well known that a copious deposition of earthy phosphate is frequently produced in urine by the application of heat, and it is also known that occasionally, but much more rarely, the addition of dilute nitric acid to urine will cause a precipitation of uric acid so abundant and rapid as to simulate that produced by the same reagent in albuminous urine; but, except in the instance I am about to relate, I never met with a specimen in which these properties coexisted, nor am I aware that any such is on record.

On the 15th April, 1854, I received from Dr. Johnson a specimen of urine for examination, with the query, "Is it albuminous?" I found it to be slightly acid, its specific gravity was 1.023, and it contained a moderate proportion of urea. On standing for some time it deposited a large amount of urate of ammonia tinged with purpurine. Having filtered the urine, I boiled a portion of it in a glass tube, when a copious deposit took place, appearing amorphous under the microscope, and dissolving instantly on the addition of a drop or two of dilute nitric acid. The solution thus produced was not, however, quite limpid, and a considerable precipitation of uric acid soon occurred. The addition of dilute nitric acid to another portion of the filtered, but unboiled urine, produced an instantaneous and copious precipitate, which redissolved on the application of heat.

^b The existence in the fæces of soaps of lime and magnesia, formed by the decomposition of a portion of the fat met with in the intestinal tube, may assist in explaining the occurrence, under certain circumstances, of similar compounds in the urinary organs. See a review, by the writer, of the second part of Bidder and Schmidt's work, "Die Verdauungssäfte und der Stoffwechsel," in the British and Foreign Médico-Chirurgical Review for April, 1854, vol. xiii. p. 395.

^c Vol. ii. p. 454.

^d Urinary Deposits. 4th edition. London, 1853. Appendix, p. 446.

This precipitate, when subjected to microscopic examination, was seen to consist of a great quantity of minute crystals, mixed with a less number of much larger ones; of the latter many were lozenge-shaped, and presented a pale violet or puce colour. This coloration was evidently owing to the action of the nitric acid. Some of these crystals exhibited the cylindrical appearance so frequent in crystals of uric acid, and as these were seen in larger bulk, their colour was a much deeper violet. These experiments were, of course, made previously to the deposition of the large amount of urate of ammonia I have mentioned.

This case illustrates the necessity, in testing for albumen, of applying heat and nitric acid to the *same* portion of the fluid; the separate application of these agents to different specimens of the same urine might lead to an erroneous inference.

Dr. Johnson informs me that this urine was drawn off, between three and four weeks after her confinement, from a lady, who subsequently suffered from a severe attack of puerperal metritis, from which she is now convalescent. On account of a tendency to diarrhoea she had been obliged to take opiates constantly, which appeared to produce retention of urine, for as soon as the opium was discontinued, the bladder began to act. Prior to delivery the patient had œdematous swelling of the hands, feet, and face, and it was reported that, at that time, the urine was albuminous. The lady was not then, however, under Dr. Johnson's care.

On the Truces and Cure of Venereal Affections. By Dr. PIETRO GAMBERINI, Assistant Physician to the Hospital of Saint Ursula, at Bologna.

IF there be a contagious affection, accompanied by many strange and equivocal circumstances, it is certainly the syphilitic taint, whether we consider its protean modes of appearance, or the fact of its embracing an indefinite period of sufferings, which once passed give rise to the flattering conviction in the patient's mind that he has no more to dread, but are followed by the day which cruelly destroys his golden dreams of happiness and health.

When the violence of small-pox is subdued, the disease may again be encountered almost without fear of injury; the individual who has passed through measles, scarlatina, or the perils of petechial typhus, may, in general, reckon on future immunity from similar invasions of these scourges; but he who has been freed from the primary effects of the venereal poison remains capable of contracting it anew; while in those who have been cured of the consecutive lesions under the form of any syphilitic affection, it is rarely difficult to cause a relapse of the malady, even for an indefinite number of times. This circumstance, which is merely the expression of a fact of daily occurrence, clearly demonstrates that while the time of action of the other contagions is of short duration and uninterrupted, that of the venereal virus is, on the contrary, irregular and indefi-

nite; relapse of the former is a most singular exception, the reappearance of the latter is the almost characteristic rule of its nature; the first are, at the same time, evils and remedies, the second is purely an evil, because it does not prevent the contraction of a new infection.

Syphilis, then, is composed of repeated possible and easily occurring primitive infections, and of constitutional sequences, which latter undergo most easy and almost constant relapses before being completely eliminated and destroyed. Studying these relapses with attention, I was led to observe a series of circumstances which I deem worthy the attention of the practical physician and surgeon, whether with a view to the right interpretation of the facts, or to the judicious selection of the several remedies suggested in the various forms of syphilitic affections.

Between the several recurring appearances of the venereal disease there is an indefinite, irregular, and variable period, which may be called *a truce*; this it is which indirectly leads to cure, and hence it is by many confounded with the cure itself. I now propose to speak of the causes, importance, and description of this truce, studying it at the same time under its extrinsic and direct relations, and under its concomitant pathological and therapeutic conditions.

Primary syphilitic lues has no truce; chancre and blennorrhagia, when they have once manifested themselves, generally run through their proper phases to the complete cessation of all characteristic symptoms, without the occurrence of intervals worthy of the name of truce; for by this term, used with reference to this disease, I mean the complete cessation, for a given time, of everything constituting a visible and sensible phenomenon of the venereal affection, whatever it may be, which was in progress. It is true, that occasionally the urethral discharge or the ulcer will, after they have disappeared, subsequently seem to return, but a little attention will in such instances show one of two things, either that the ulcer had not completely cicatrized, and the discharge had continued in a masked form, or that they now constitute a symptom of constitutional syphilis; in the first case, cure was erroneously supposed to have taken place; in the second, we have no longer to deal with a local or primary syphilis, but with a secondary and constitutional taint.

What I have said of the two most common forms of venereal disease is equally true of inflammation of the inguinal gland or bubo; and this will be more apparent if we recollect that this disease can simulate the truce, in the sense adopted by me, only in the eyes of a very careless observer; for as long as the seeds of disease remain in them, we never find the glands of the groin in their true physiological state, but turgid, troublesome, and irritable; this is exemplified in the rare cases of sluggish buboes consecutive to any antecedent primary ulcerous infection.

The truce, then, as I employ the term, is an exclusive and characteristic phenomenon of constitutional syphilis; the medical man

sees it in its true light; the non-medical man mistakes it and describes it by the ancient adage, that he who has been tainted with syphilis is always cured except the first time, a popular saying, which, like all the proverbs inherited from tradition, has its side of truth.

The truce, so far as I have had opportunity to observe, is at one time spontaneous, at another artificial, that is to say, it is sometimes the consequence of treatment employed while the disease was manifest. And first, in reference to the spontaneous truce, I will say, that it is the basis of the several manifestations of constitutional syphilis, or, in other words, of the several successions of venereal affections which almost constantly exhibit themselves in syphilitic patients; in fact, before constitutional lues emerges from the local, an indefinite lapse of time takes place, which serves to prepare that modification of the venereal poison which is to act under the form of confirmed lues; and this is the true truce, inasmuch as, notwithstanding the presence of the pathological agent, the patient, at least in the generality of cases, does not suffer any inconvenience; if the reverse be the case, the truce has disappeared to give way to the first symptoms of the now developed disease.

Another kind of spontaneous truce takes place when constitutional venereal symptoms exist in cases not yet subjected to any curative treatment.

I shall now proceed to give a faithful description of the phenomena spontaneously produced in the natural course of the disease.

Ricord has endeavoured to establish, by almost constant laws, the well-known theory of venereal successions, distinguished, as every one is aware, into primary, secondary, and tertiary. I will not dwell on the inexactness of this division, because, in other writings, I have opposed it by reason and, what is still better, by facts. I say, then, that the truce serves at one time to reproduce the passed syphilitic phenomenon, and at another gives origin to a different venereal affection, after which, a truce succeeding is again followed by the manifestation either of the first phenomenon, or of another totally different, which may, in some cases, assume a form so mysterious and equivocal as to embarrass the most experienced practitioner, the most philosophical and analytical physician. This is the commencement of the protean syphilitic lues, which may appear with the most characteristic sign, or may sometimes assume the darkest disguise, of that lues which passes silently and insidiously from the most simple and most accessible to investigation of the organs or tissues, to the most complicated and most deeply-seated viscus. The following cases may be quoted in illustration of what I have advanced:—

CASE I.—Angelo R., having contracted a primary ulcer, had, after eight months, a troublesome ostealgia, which lasted fifteen months, after which a truce of half a year occurred, followed by an extensive maculated syphilitic eruption, which yielded in a short time to the use of mercury.

CASE II.—Clement L., after repeated infections with primary ulcers, was attacked with a syphilitic papular eruption, which was not treated, and lasted two months, when it completely disappeared. A similar occurrence took place during several springs, lasting each time about two months—no medical treatment having been ever employed. Finally, the papular became converted into pustular syphilis and palmar psoriasis. A judicious employment of mercury put a stop to the annual recurrence of the disease.

CASE III.—Antonio P., some months after having had a primary venereal sore, was attacked with mucous papulæ, which were left to themselves, and slowly disappeared; the patient, not having perceived any trace of syphilitic taint for some time, thought himself wholly free from it, when a mild nocturnal arthralgia came on, which soon went off without treatment, leaving a truce of about four months, at the end of which time the above-mentioned papular syphilis reappeared.

CASE IV.—Luigi B. contracted a chancre, followed by a suppurating bubo. About seven months after he got rid of these affections, a venereal ulceration appeared in the throat, which was not treated; nevertheless a truce of some months took place, at the end of which an extensive pustular syphilitic eruption came out, which, likewise left to itself, gave way to a second truce; this was followed by osteocopic pains; these, after repeated truces, finally ceased under a treatment consisting of the administration of mercurials, followed by the use of iodide of potassium.

Were I to continue the enumeration of similar cases, I should do no more than narrate facts of common occurrence in every venereal hospital; I shall, therefore, simply state that confirmed venereal lues is exceedingly versatile in its manifestations, and although, in very many cases, it observes the laws of succession announced by Ricord, the phenomena, which are thus occasionally successive, in several instances vary, appear simultaneously, or are transposed until, the entire organic system becoming contaminated, that general form of syphilis is developed which is called venereal tabes, or consumption, a condition, in one instance, easy of recognition, from the characteristic nature of some attending constitutional symptom, while in another it will be rendered obscure by the masking of its phenomena; to meet which latter case some employ the term pseudo-syphilis, signifying thereby an amorphous disease devoid of decided colouring, or well-marked fundamental characters. This language seems to me to be inexact, for, in the complex disease, either we have the traces of syphilis mixed with complications, or such traces are wanting, and are rather suspected than demonstrated; and yet, in both these instances, the word pseudo-syphilis is employed. Venereal lues is always identical with itself, and therefore, when it exists, maintains its proper pathological and therapeutic essence, whether its symptoms be isolated, or whether they concur with other morbid casualties. The term pseudo-syphilis, then, appears

to me to have been unhappily chosen, and to represent an idea which is too much opposed to true syphilographic philosophy.

But to return to the subject of the spontaneous truces of constitutional syphilis, that is to say, of those truces which occur in patients who have adopted some form of treatment, observation has shown me, that the affection which most readily admits these truces is osteocopic pain, which goes and comes repeatedly and indefinitely for months and years, at one time in a masked form, and at another with the most evident characters; the former is usually mistaken by careless observers, and confounded, to the patient's injury, with the effects of a draught of air, checked perspiration, or atmospheric humidity—an etiology acceptable enough to those who, while they lament the inefficiency of the means employed to relieve their pains, do not wish to believe that they labour under a venereal taint. But, after a repetition of such truces, we have at length not merely osteocopic pains to deal with, but a material lesion of the bone, periostitis, or exostosis—affections immediately resulting from osteitis, when a vigorous and suitable treatment has not been adopted.

In reference to the frequency of easily occurring truces, syphilitic catarrh, and then affections of the skin, come next to ostealgia; maladies which, when mistaken, are referred to acid humours, catarrhal diathesis, &c. As to the affections of the skin, although they sometimes recur with varied aspect, they do not, in my opinion, present the variety that might be expected, or such as occurs between ulcers and nodes, rheumatism and periostitis.

If we admit these truces of the phenomena of constitutional syphilis to be established, we should next inquire if they proceed from a natural law of syphilis, or from causes extrinsic to this disease.

In answering this question I shall give the results of ten years' observations. I have seen patients who remained infected with chancres or blennorrhagia fall very rapidly into a state of confirmed syphilis, which, without any interval or truce, brought the patient into a condition of health so bad as to threaten to destroy life if medical aid did not quickly interpose; this uninterrupted course of lues at one time proceeds slowly and mildly; at another, it advances with quickness and severity; in the former case the morbid phenomena are usually variable and superficial; in the latter, on the contrary, they are constant, deep-seated, and different forms of the affection often coexist. The cases of such an unbroken chain of syphilitic affections are not frequent, and are chiefly observed in subjects of a lymphatic or decidedly scrofulous habit, in whom venereal symptoms are usually both severe and obstinate, in addition to which a circumstance occurs to complicate the treatment, which is, that while the scrofula appeared in consequence of the syphilitic infection, and was, therefore, subjected to mercurial treatment, a period arrives when the scrofulous symptoms continue, while the venereal poison which excited and produced them has been subdued and an-

nihilated; a period in which, I say, the disease has been converted from syphilitic into simple scrofula; and while it is extremely difficult to lay hold on this pathological moment, it is, at the same time, necessary to recognise it in order to alter the treatment, for I have, in the generality of cases, observed that while mercury was exceedingly useful in syphilitic scrofula, it was most injurious when the latter disease was removed from the influence of the venereal virus. The following is an example of a remarkable circumstance connected with these cases:—A form of the disease above mentioned was improved by mercurials, until it was found necessary to suspend their use, which was done with the greatest benefit, and with almost complete cure of the glandular disease, when the latter again set in, the glands enlarging, and necessitating a return to the use of mercury, which duly administered completely removed the symptoms.

Having alluded to the instances in which lues never intermits or forms a truce, I will say that in the greater number of cases, I mean those which are treated for the first time, syphilis appears in connexion with truces in the sense I have laid down, independently of the temperament, constitution, and habits of the patient, and without our being able to refer these truces to the influence of the seasons, of the climate, or of the atmosphere; for they evidently have their origin in the protean developments of syphilitic symptoms which are insidiously preparing to exhibit themselves at the proper time with characters at one time sensible, at another visible, and again, with both these qualities combined. We may add, that such truces are sometimes, as I have elsewhere pointed out, far from being the fulcrum of the numerous and varied evolutions of the disease, only the corner-stone of the same venereal affection which continues unchanged and constant; just as, during the repose of the vegetation of trees, the latter become deprived of their leaves to be in due season reinvested with the same: the plant is in our case constitutional syphilis, while its symptoms may be compared to the leaves.

If we compare the number of syphilitic patients who enjoy the benefit of spontaneous truces, with that of those who have not such intervals, we shall find a decided preponderance of the former class. The truce then of constitutional venereal symptoms is one of the special characters of syphilis, a disease which, while it lasts perennially within the system, becomes concealed and latent, to be subsequently called into action by particular circumstances. From the results of my own observations I should say, that the causes of the spontaneous relapses of lues are usually the following: the change from the cold of winter to the warmth of spring; drinking and bathing in mineral waters, and especially sea water; living in a damp house or situation; want of sufficient diaphoresis; want of wholesome and nutritious food; traumatic lesions in any part of the body which seem to elicit the latent venereal taint; in fine, all that tends to weaken the physical power by impoverishing the system, or serves to depress the strong qualities of the mind by casting the individual into the abyss of the passions, which deprive the

body of energy more quickly and more certainly than physical evils, consuming it slowly and uninterruptedly^a. The foregoing etiology receives confirmation from the observation that most destructive syphilitic actions take place in those who are subjected to the causes just now enumerated. So that I am persuaded that when we have to do with slight venereal disease, accompanied by truces, and can in such cases remove all that tends to disturb these truces, the disease will slowly but eventually be annihilated by the force of the healing powers of nature, which, when well seconded in its operations, constantly tends to repel from the system all that is extraneous, hurtful, or prejudicial; but it is, in my opinion, because no one, or scarcely any one, is capable of making the absolute sacrifices demanded, of following the rules, and obeying the directions of the physician to the full extent required, that we do not see the spontaneous cure of syphilis, which, if we reflect, is not in the generality of cases dangerous, though it gives us reason to fear for the seat in which it manifests itself, the organ it attacks, the viscus it injures. In fact, where is the danger in cases of affections of the skin, of alopecia, of rheumatism, and such like? And where, on the other hand, can we find ground for a favourable opinion in cases of ozæna, of ulcerated throat, or of chronic osteitis?

I have said that I believe in the spontaneous cure of the slighter forms of syphilis; I think differently, however, when this disease is severe and threatening, because in such cases, far from ceasing in consequence of the beneficial interference of nature, it only runs a ruinous and fatal course, if we do not oppose its progress with all the therapeutic means which have been sanctioned by the experience of ages as fittest and most useful. Woe to the practitioner who, trusting in a temporary lull, would stand idly looking on; he should find nothing but mutilation, ruin, and even death, reproving that too far stretched expectation carried unmeasuredly into the treatment of diseases, as if nature would suffer herself to be ruled by the coaxing look of the inactive physician.

The natural and spontaneous cure of constitutional syphilis is, then, an event in its nature probable, and by no means repugnant to the essential character of the venereal contagion; but such a result is, strictly speaking, not to be obtained, at least dating from the commencement of the disease, for whoever suffers from such a taint will not fail to make attempts to get rid of it, and medicine is within the reach even of the poorest,—for these reasons I do not consider that we have any certain, unexceptionable, and unequivocal evidence of the absolutely spontaneous cure of general syphilis. I know that there have been instances of slight secondary venereal affections which got well under the use of ordinary remedies, neither mercurial nor antisymphilitic; but, as was to be expected, such medicines had little or no directly or radically curative effect, as was

^a Dr. Gamberini here omits the most important and manifest of all these causes, namely, in the female sex, the occurrence of pregnancy.—ED.

evidenced by the reappearance from time to time of the morbid phenomena; still these repetitions took place with a constant decrease of symptoms and annoyances, until it was no longer possible to detect the presence of the venereal virus in the individuals affected. But I do not call such a cure spontaneous, for here we had the interposition of a medication, which, although not specific, had an indirectly therapeutic influence, as I shall show in speaking of the relations which exist between the treatment, truces, and cure, of constitutional syphilis.

In admitting cases of syphilis as spontaneously cured, it is necessary to be exceedingly circumspect and watchful, because, while there are authors who relate such instances as happening by preference in southern climates, there are others who deny their occurrence in these localities; a discrepancy which, in my opinion, proceeds from the too great value set upon the truces, and from the relief obtained in the generality of cases by the patients in the hospitals; a ground for induction which is scarcely safe, because we usually have only a temporary, and often an incomplete and equivocal, observation of these patients: when, therefore, we speak of decided and positive relief of syphilitic affections, we must recur to private practice, as affording opportunities of watching patients for many years, and of tracing the steps and attending circumstances of a taint so malignant and protean as that of syphilis.

From the facts I have observed it appears to me that the probably spontaneous cure of syphilis takes place more easily in those cases in which the general taint has sprung from urethritis; a primary form, which, principally attacking the osseous system in its secondary effects, is less hurtful to life, because it affects parts which have less influence thereon, while at the same time it occasionally leaves inevitable and indelible traces of its passage. So that if constitutional lues, arising from chancre, is less disposed to a spontaneous cure than that derived from virulent urethritis, I think (and I am supported in this opinion by the results of clinical observation), that this proceeds from two causes: first, because lues from chancre more easily invades those parts of the body which are in an important and necessary manner connected with life; and, secondly, because this form is, on account of its severity, seldom left to itself, but is, on the contrary, subjected to at least some treatment, but most frequently to the use of a remedy of known antisymphilitic power, in consequence of all being agreed on the essential venereal value of primary chancre, while not a few think differently as to the fundamental character of blennorrhagia: an idea which reason and experience ought to banish from syphilology, as it is opposed to both; but I shall not here dwell on this point, because I think I have said enough, and adduced sufficient proof of my views in my various writings on clinical syphilis^a.

^a From these remarks it is evident that the author is a believer in the unity of the syphilitic poison, and that blennorrhagia is one of its forms.—Ed.

I now proceed to speak of the truces and cure of constitutional syphilis, in reference to the specific and non-specific therapeutics of that disease, with a view to discover the value of the various and opposite medicines which have been recommended.

In the first place, I think I ought to remind my readers that the curative plans proposed in confirmed lues are two in number, the so-called antiphlogistic and derivative treatment, and the established specific mode, consisting in the use of mercury and of the preparations of iodine. The followers of these two methods have sought to support their respective systems with arguments and facts, occasionally not omitting the degrading custom of calumny, insult, and falsehood, as if science ought to mix herself up with the trivial and passing habits of society. But as truth is sole and indivisible, the moment arrives, in spite of all efforts to keep it back, when it must be made manifest. The *true* therapeutic remedy of syphilis is mercury, and its principal substitute is iodine; the *false* is all which is not mercury or iodine.

It now remains for me to show how the syphilitic truces have confirmed the pretended value of remedies which are neither mercurials nor preparations of iodine, and that not only has injury thus arisen to syphilographic science, but, what is worse, what detriment patients generally receive from such medicines.

From what has been above advanced, it appears to be demonstrated that constitutional syphilis has in itself the source of the spontaneous truces, or, as I have said, the temporary cessation of its material and visible phenomena. Now, if a purely antiphlogistic medication be employed on the invasion or during the continuance of some venereal affection, and if its use be followed by a truce, will it be fair to deduce, and what is worse, to maintain, the curative value of the treatment adopted? In running over the histories told us by the advocates of this species of therapeutics, we can lay hold on nothing positive, for either they speak of relapses, or they leave their observation incomplete, inasmuch as they do not continue to look after the patient who was the subject of their report, and the ease with which relapses of syphilis take place is known to all. I may add, that not only I, but many practitioners have had to treat not a few of the patients who were said to have been cured with antiphlogistic remedies, and whose cases were described in the periodicals; and yet in these cases the inadequacy of the plan was evident, and we smiled at the credulity of the reporters. An involuntary mistake, proclaimed with the sanction of a respectable name, drew with it error on the part of those who rested on that name; and thus truth was set aside until it found reception and support from some other physician whose love of truth made him fearless of encounter.

Were I to quote clinical facts in support of my assertion, I should but carry vases to Samos and bats to Athens; it is enough to read what the antiphlogistic method has done, and is doing, in the treatment of constitutional venereal affections to induce us to bewail the lot of the patients who were the subjects of it: those who saw a ray

of hope in the appearance of the truces quickly fell into a worse physical condition when those truces ceased,—an unfortunate event founded on two circumstances, the first based on facts observed by me as well as others; the second on the following,—that syphilitic patients, who are much restricted in their diet compared with those who live freely, are generally in a worse condition than the latter; and further, those who are kept on a limited diet while under treatment do not long bear it; while the reverse is the case where the opposite plan is adopted; and moreover, I have witnessed several different antiphlogistic modes employed with syphilitic patients for a very long time, at the end of which a patient rather reduced in condition would be found with apparent cessation of the disease, and would be declared cured, although the supposed cure was no more than a truce, and when the case was left to itself the disease would reassume its bad type, with symptoms at one time purely visible, at another exclusively sensible, and still more frequently, with characters of both kinds. It appeared as if the virus, rendered inexpressive though want of organic energy, became stunted, or, as it were, masked, to increase and embody itself when the fibre should regain its natural strength.

I am aware that the anti-stimulist does not acquiesce in the existence of syphilitic truces, and will suggest to me the necessity of persevering with his method, so as to combat and completely destroy the masked or stunted appearance of syphilis, which I have above supposed. But if we continue to withhold stimulants, and to depress syphilitic patients, what I have repeatedly observed will happen, that is, in the most favourable cases we shall be obliged completely to suspend this treatment, either on account of the derangement of the stomach, or of general exhaustion; or, what is more likely to take place, a cessation of the truce will supervene, leading to the severe and threatening form of syphilis, which endangers the patient's life, unless mercurialization be quickly interposed, which sometimes, although employed thus late, restrains, it is true, the virus a little, but does not prevent the morbid results which the venereal poison has imprinted on the system of the unhappy sufferer.

The truce then is, in such an instance, the illusorily beneficial effect of the antiphlogistic treatment, of that treatment on which some obstinate systemists still rely, but which is rejected by the faithful observer of the morbid results of lues venerea.

What I have said applies equally to the various modes of treatment which completely exclude the use of mercury: the employment of the preparations of gold and of silver, the acids, concentrated diaphoretic syrups, &c.; these in most cases induce truces, which are too readily denominated cures; I say advisedly truces, reflecting that if such remedies were really of use, they would not be condemned to oblivion, or they would be frequently employed, which they are not. I may add, that to my own knowledge, many of the persons so treated were in the end obliged to have recourse to the

use of the sovereign antisymphilitic—mercury. I will certainly not venture to deny that in some instances cure has followed the employment of these remedies: but I will add, that in the great majority of such cases mercury had previously been exhibited, a fact which greatly lessens the absolute value of those non-mercurial medicines. It is the truces which mislead, and they do so the more readily the longer they are protracted.

It is now time for me to speak of the syphilitic truces which succeed the use of the preparations of mercury and of iodine; and in the first place I will remark, that the true, the most frequent, and most complete truces are those which are subsequent to the administration of these medicines; this appears to me to be what we should expect, seeing that we now have the disease in conflict with its true remedy, which, if it does not succeed in subduing or destroying, is certainly capable of restraining and circumscribing it. If then the truce be nothing more than a temporary suspension of the malignant operation of the constitutional venereal virus, if this truce be a natural phenomenon of lues, it follows that all which tends to oppose the malignity of the syphilitic poison must lead to a more easy and ready truce, though it may not effect the cure of the complex pathological condition; mercury and iodine combine this result, and thus, with the greatest facility, induce the truces, and in due time the cure of syphilis: which cures, too quickly proclaimed, or too eagerly seized on, often prove to be no more than complete and prolonged truces.

When mercury and the preparations of iodine are employed for the cure of some syphilitic affections, not before subjected to treatment, they do not effect their object on their first administration, though this be carried on with all possible caution and regularity; a favourable truce will, however, be obtained, a stable and perfect cure will be had, I venture to say, but rarely, and only as an extraordinary exception. In truth, I frankly confess, I have observed that the almost invariable rule for obtaining a radical destruction of syphilis is to repeat the anti-venereal treatment twice, thrice, or even oftener, to each of which repetitions a truce succeeds more manifest and more complete than the preceding one, which truce is a direct step towards the attainment of perfect cure, for it appears that syphilis cannot be eliminated from the system otherwise than *à reprises*, and by a decreasing scale, and not instantaneously or without undergoing, as it were, a dilution, and gradual wearing out.

There are some constitutional syphilitic affections which undergo truces with more readiness than others, and therefore relapse with greater frequency and facility. Amongst these, osteocopic pain holds a very prominent place, which, if it is treated with mercury, or with iodide of potassium, is latent for a time, then returns, again undergoes a truce, and finally returns, it may be repeatedly, to harass the patient. Before iodide of potassium was recommended as a specific in this and other so-called tertiary forms of venereal

disease, mercury was employed certainly with profit, still ostealgia ran its course. When the ioduretted preparation came to be used in such cases, it was asserted that the relapses of osteocopic pains proceeded from the use of mercury, and that, therefore, mercurials ought to give way to the iodide of potassium, the sovereign remedy of tertiary syphilis, as that which cured it *tuto, cito, et jucunde*. Then medical men were heard to narrate its prodigies; the journals proclaimed its miracles; prodigies and miracles which were often but truces, entirely ceasing in time: and this statement is so true, that the very advocates of the iodine came to a more moderate conclusion, which Diday of Lyons, at the present day, expresses thus:—"The iodide of potassium palliates these pains more frequently than it cures them without relapse; but to compensate for this, the relapse, though it be frequent, does not fail to obey the remedy." What the French surgeon says of osteocopic pains is also applicable to periostitis, nodes, exostosis, muscular contraction, and such like affections, in which I every day see how the iodide of potassium exercises but a moderate effect, for if we depend on it alone, a cure only takes place after relapses, and is but rarely obtained as decidedly as when mercurials have been used. From the common occurrence of such instances in the Hospital of St. Ursula, we have been led to frame the following therapeutic axiom,—that the iodide of potassium is the best substitute for mercury, but that the latter is the antisiphilitic remedy *par excellence*, and that it increases the beneficial effect of the iodide, if it has been administered before it.

Every medical man who has had the care of many syphilitic patients will have observed that sometimes the venereal symptoms in those who are subjected to mercurialization, or to the action of iodides, diminish as if by magic; as on the other hand he will have met cases in which a form of disease, slight, or at least appearing to be slight at the commencement of the treatment becomes aggravated in proportion as the latter advances. The former occurrence might very well be attributed to the circumstance of an impending natural truce of the lues, as the second might be explained by the patient being in one of those intervals in which spontaneous syphilis arises and increases; so that two elements compose the desired state of well-being of the patient, the spontaneous truce, and the use of mercury, which fructifies and perfects it, while at the same time it combats and annihilates the general taint; in these cases, however, such rapid improvement is usually not of long duration, or at least it does not afford a certainty of complete cure, except in patients who have already undergone repeated and regular anti-venereal courses of treatment. Two elements, moreover, in my opinion, determine the second fact above mentioned, that, namely, of the aggravation of the syphilis by the exhibition of mercury; the first being, as I said, the natural recurrence of the venereal exacerbations; the second produced by the energetic and powerful action of the mercury, which, like a lever, tends forcibly to expel what before lay dormant and concealed: the truces which ensue in these

cases are usually more complete and more lasting, and we may even sometimes hope for a real cure, for such struggles in general take place only in patients who have scarcely employed any true venereal remedy, or rather, in those who for the first time labour under a syphilitic taint. Recapitulating the axioms just now laid down, can the physician with certainty decide when he has to deal with a truce, and when with cure?

The lover of truth who excludes from syphilology the idea of the unity of constitutional venereal infection, or, as some will have it, that a man can receive a general syphilitic contamination but once in the course of his life, must confess, in view of the clinical facts, that the things I have hitherto said in reference to truces, whether spontaneous or produced by remedies, are liable to different interpretations; for the fact of constitutional lues produced by a single infection is one thing, and that of an individual who has suffered several infections, whether these be chancreous or virulently blennorrhagic, is another. I have watched the course which constitutional syphilis ordinarily presents in those who have repeatedly had primary venereal disease: here, too, truces were observed, but they were rarely so perfect, and seldom relieved the patient so much as is usually the case in syphilitic patients who have suffered but a single contagion. On referring to my registers of syphilis, in which I have noted about a hundred cases of disease, I have been able to deduce the following propositions in reference to repeated primary syphilis:—

1. The truces retained in a greater or less degree some visible or sensible trace of constitutional lues; they were not complete truces.

2. The new local and primary contamination, quickly becoming a constitutional venereal affection, presented an obstacle to the occurrence of the truces, because a sort of uninterrupted chain of syphilitic sufferings was thus formed, in which, instead of the truces, only a temporary decrease of disease took place, or, as in some fortunately rare cases, a localization ensued so obstinate as not to yield to any treatment, in consequence of which the disease continued for an indefinite time. In every venereal hospital incontestable proofs of this statement may be found.

3. Repeated primary venereal infections give rise to a syphilitic condition so protean and variable, to epochs so uncertain in their appearance, as to demand the greatest diagnostic skill in the medical attendant, and to require extreme endurance in the patient. In these cases, however, we finally though tardily succeeded in obtaining perfect truces, that is to say, provided a long time elapses without the reception of fresh infection, and that judicious and repeated anti-venereal remedies have been employed.

And why should we be surprised at the fact of repeated venereal infections favouring a continued and variable succession of general syphilitic phenomena? If the superadded and repeated concurrence of morbid causes serves in the generality of instances to greatly increase the intensity of the disease to which they give rise, why should

the multiplied primary venereal infection prove an exception? If our system were no longer capable of receiving the influence of fresh venereal infections when constitutional syphilis had already been developed from a preceding contamination, why does it not behave when it comes in contact with the venereal virus as it does with the virus of small-pox when this has previously had an opportunity of producing its effect? If the absence of local variolous development from infection indicates that our system is no longer in a condition to appropriate the material of the contagion from which the disease of small-pox proceeds, I would say that the repeated development of the venereal virus exhibits the susceptibility of our system to imbibe the poison which causes a repetition of constitutional syphilis; a susceptibility which is in my mind established, as I have observed that as a general rule a manifestation of constitutional lues sooner or later succeeds to a new chancreous infection. The poison of these new ulcers going to form constitutional virus explains, in my opinion, the derangement of the truces in cases of repeated contagion, whether blennorrhagic or ulcerous.

Having noticed these points in reference to the truces of general syphilis, and having briefly demonstrated the uncertainty of mode and time of these truces, it remains for me to speak of the perfect cure of constitutional lues. That this taint is fully eliminated from the system of those who have suffered from it is demonstrated by facts so clear and full as to remove all doubt from every one who does not delight to wander in the region of clouds and sophisms; but what greatly embarrasses the physician and torments the patient is the uncertainty of the period at which true cure takes place. This unfortunate circumstance constitutes a new, and I venture to say an exclusive character of syphilitic contagion; for while the other contagions with which we are acquainted form an uninterrupted morbid chain, terminating at a time approximately known, the venereal infection, on the contrary, runs a broken course, and ceases at unknown or doubtful epochs. Nor is it enough to blame the insufficiency of the treatment, or the intolerance of the patient, in many cases the most perfect therapeutics refute such appeals, though these are very often just and incontestable, so that we are forced to attribute the defect to the nature of the syphilitic contagion itself. To the physician, therefore, is conceded the hope of effecting the cure of constitutional syphilis, as he has the means of accomplishing it; but he is denied a reasonable and absolute certainty of attaining it every time that he makes the attempt; the most that is granted him is that, when he and the patient act with prudence, nature interposes beneficial truces of an ordinarily decreasing morbid character, which, without pointing to a certain and immediate cure, allow the experienced practitioner to perceive that, by persevering in a judicious medication, he shall attain his desired object. The cure, then, of constitutional syphilis is only obtained under special circumstances, and with the aid of time, which latter is not, however, as the superficial observer pretends, necessarily a

space of three years, but may, on the contrary, be shorter or longer according to circumstances, as I have elsewhere pointed out, but which is chiefly influenced in its duration by the occurrence of repeated primary infections.

What I have said describes, if I mistake not, the natural course of general lues, so far as this has been as yet established; should the truth of the disputed subject of syphilization be ever established, what I have advanced would necessarily be modified and partially refuted.

I shall close this essay by reducing its substance to the following heads:—

1. The syphilitic taint, in addition to the intrinsic differences which distinguish it from other contagious diseases, possesses that which I denominate *truce*, or temporary cessation of the visible and sensible phenomena of the malady.

2. The truce takes place either spontaneously, that is, as it were, by a peculiar law of constitutional syphilis, or artificially, from the operation of therapeutic means.

3. The occurrence of the truce has induced a belief in the therapeutic value of the methods of treatment which exclude mercury, which latter medicine has been shown by experience to be the true remedy for syphilis.

4. These truces, whether natural or artificial, lead, in a period of uncertain duration, to cure by means of relapses of a pathological value which is usually a decreasing quantity: this is ordinarily the case if the constitutional disease was the effect of a single infection; the contrary generally obtains if the primary venereal infections were repeated.

5. The cure of confirmed syphilis is accomplished only by means of the truces; it therefore requires an indefinite time for its accomplishment, and demands a proportionally vigorous and repeated treatment.

In conclusion, I have only to observe that my design was not to treat of subjects which were either new, or previously unobserved; but merely to call attention to the contemplation of facts, namely, the truces, which misinterpreted would compromise the character of the physician, and sadden the existence of the patient.—*Bulletino delle Scienze Mediche di Bologna*, vol. xxiii., page 5. 1853.

On the Treatment of the Agony. By PROFESSOR FORGET, of Strasburg.

NEVER despair of a patient while he gives sign of life! If all practitioners were persuaded of the importance of this precept, they would find themselves less frequently deceived in their fatal prognostics; they would less frequently experience the embarrassment of, so to speak, seeing the patients revive whom they had consigned to impending death; while ignorance and charlatanism would more

rarely reap credit at the expense of science, by restoring life and health to those who were deserted, as it is called, by the physician.

This is applicable to all acute and chronic diseases actually in a state of evolution, and of which experience has demonstrated that the issue is almost constantly fatal. For example: how many convulsive attacks do we not observe in children, which seem to announce a fatal cerebral fever, and which nevertheless quickly terminate in recovery. How many typhoid affections are there attended with the most serious symptoms over which art and nature triumph. How many dropsies, consumptions, and even so-called cancers, deceive the fatality of the prognostic. I might bring forward a large number of examples of these apparently desperate diseases, the issue of which has contradicted the best founded predictions. Perhaps I may hereafter draw up a list of these rare cases from my personal experience; but it is to a different branch of the subject that I am anxious at present to call the attention of my readers.

If the public, and even some physicians, always maintain a certain amount of hope of witnessing the cure, or at least the protraction, of the most serious diseases, so long as the catastrophe is not closely imminent, this feeling ceases when the disease has arrived at the point in which the vital principle is about to become extinct, when respiration, circulation, heat, feeling, motion, exist but in feeble traces, in a word, in the confirmed agony.

What is the agony? Etymologically the word signifies combat, and by application, a contest between life and death^a. Strictly speaking, disease, in general, is nothing else; but conventionally we understand, by agony especially, a very unequal struggle, in which death has manifestly the superiority, in which its triumph is as sure as it is close at hand. Well, it is exactly this certainty and this proximity which constitute the problem, and which sufficiently frequent recoveries should convert into mere probabilities.

In spite of the criticisms which have been brought forward against the physiology of Bichat, his triple life, imitated by that of Barthez, is still received; and it is always correct to say that a man dies by the brain, by the heart, or by the lungs. Certainly death may strike out for itself a passage by any organ, even the least important; it may enter at all points of the economy; but each of these branches leads eventually to one of the centres I have mentioned. We cannot even except the stomach, the fourth branch added by Broussais to the tripod of life, which, in the teaching of that reformer seemed even to aim at suppressing the three others. We might still more simplify this mechanism, and demonstrate in transcendent physiology, that death really takes place only by a single organ, the brain; for it is the cessation of the nervous influx which formally and radically constitutes the cessation of life, even when death appears to emanate from the heart or lungs, for syncope and asphyxia act, definitively, only by suppressing innervation.

^a [“Agony, the last contest between life and death,” *Johnson*.—Tr.]

This digression on the essence of the agony is not entirely irrelevant to the subject on which we are engaged, for we shall see from facts, that without neglecting other organs, it is particularly by attending to the innervation, or at least to one of its principal attributes—sensibility, that we succeed, in most cases, in withdrawing those who are in the agony from the embrace of death.

The diagnosis of the agony, so far as it is to end necessarily and quickly in death, cannot be accurately defined, but proceeds from the power of appreciation acquired from experience by most competent observers. Thus, the majority of physicians will nearly agree as to the existence of agony, that is to say, on the imminence of death in a given subject; but the agony, in general, has no absolute character by which it can be expressed. It is, in fact, only the last degree of the numerous diseases which may end in death; whence results, on the one hand, the extreme variability of its manifestations; and on the other, the difficulty of determining the moment in which this last degree is produced. A pessimist, over particular, or habitually unfortunate practitioner will consider a patient to be beyond hope, in the agony, whom another physician will trust to be able to restore to life.

The form of the agony necessarily varies, according to the organ from the lesion of which it is derived: when it proceeds from a disease of the brain, the agony is naturally and directly characterized by the early abolition of nervous acts, intelligence, sensation, and motion. It is this which takes place in the apoplexy called *foudroyante*, which, as we know, rarely produces sudden death. In serious diseases of the brain, in fact, the nervous functions are first profoundly impaired, respiration then becomes embarrassed, the pulse grows weak, the extremities cold; and it is the combination, and the threatening and increasing degree of all these phenomena, which constitute the agony.

But if the agony proceeds, in the first instance, from the circulatory centre, one of two things happens: either the heart becomes considerably weakened and even ceases to pulsate; then the blood no longer vivifies the organs, and the patient loses sensation: such is syncope, apparent death, which, if prolonged a little, becomes real; or an impediment to the circulation gradually brings about an enlargement *à tergo* of the sanguineous vessels; the lungs become congested, the nervous system is attacked with stupor; but little blood, and that imperfectly arterialized, reaches the organs; cyanosis, tracheal râle, gradual asphyxia, weakness of pulse, coldness of the extremities, supervene. Such is, in this case, the evolution of the agony.

If the lesion primarily affects the lungs, these organs become loaded with venous blood, mucus, serum, pus, &c.; as above, cyanosis and gradual asphyxia are established, the heart grows weak, coldness by degrees seizes on the extremities, the brain is smitten with stupor, the agony exists.

These several phenomena may, in each species of agony, be combined

and succeed one another in various ways. However, there are two of these symptoms which, in significance and importance, surpass all the others: the first, which in almost all kinds of agony constitutes the true thermometer of vitality, the touchstone of the physician, is the state of the pulse. However grave the situation of the patient may appear, if the pulse keep tolerably full, resistant, and regular, all hope is not lost. When, on the contrary, the pulse progressively and rapidly diminishes in fulness, consistence, and regularity, death is approaching. It is irrevocable only when the pulse, or rather the heart, has, for some moments, ceased to beat.

The second phenomenon which, though more apparent than the first, is less important, is the tracheal râle. This sign is to the vulgar the most significant, and has, therefore, been styled the agony or death-rattle. When it sets in and is heard at a distance, no one doubts the near approach of death. This râle is produced in almost all cases, whatever be the cause of the agony, whether the latter commence by the brain, the heart, or the lungs; for it results from the progressive debility, which, affecting the expulsive powers of the lungs, allows the mucus to accumulate insensibly in the bronchi; whence arises slow asphyxia, the ultimate phenomenon of almost all agonies. And yet, this pathognomonic sign of the agony, this fatal forerunner of death, may, under many circumstance, be charmed away, and bears, I repeat, less absolute importance than the failure of the pulse. I have never seen patients revive after a prolonged cessation of circulation, while I have seen a considerable number rally, in whom the tracheal rattle existed for a long time.

Loss of consciousness, coldness of the extremities, even the cadaveric countenance, have much less significant value than the two preceding phenomena.

In fine, the hippocratic face; paleness; lividity of the skin and mucous membranes; half-closed eyelids; convulsed eyeballs; muscular prostration; diminution of the general sensibility, of the special senses, and of the intellectual faculties; coldness of the extremities; characteristic sweats; laboured stertorous respiration; small, irregular, soft, slow, or frequent, intermittent pulse; difficult or absent deglutition,—such is the group of symptoms which can leave no doubt as to the imminence of death. But even when hope has forsaken him, the physician ought to feel it to be a sacred obligation to act so long as a breath of life remains, and even, in certain cases, when life appears to be completely extinct, as in syncope, asphyxia, lethargy, &c.

Few authors have specially treated of the means of combating the agony. The only one, perhaps, who has studied and developed this important subject at any length is Professor Piorry, in his *Memoir on Asphyxia from Bronchial Mucus*, and subsequently in his *Treatise on Practical Medicine*^a. He not only carefully analyzes the phenomena of the agony, but proposes a number of means calculated

^a *Traité de Médecine Pratique*, tom. iii. pp. 105, *et seq.*

to remove it. However, he appears to me to aim too exclusively at directly unloading the bronchial tubes, and among the plans he points out there are some of little value; but I will return to this subject.

We have seen that the phenomena of the agony are so connected that they might be finally referred to a single one,—want of innervation. However, art, less strict than science, permits and even requires us to distinguish several kinds of agony to facilitate description and to meet the necessities of practice. We must then, in fact, admit three species of agony:—1. Agony by defect of innervation. 2. Agony by defect of circulation. 3. Agony by defect of respiration,—the latter being very frequently combined with the two others, as cause or as effect. I shall quote some cases belonging to each of these three categories.

CASE I.—M. T., a little girl of four years of age, strongly built, of a sanguineo-lymphatic temperament, with a large head, had long suffered from an impetiginous eczema of the scalp. In 1842, having been consulted for this affection, I advised the removal of the crusts by the application of cerate, reserving further directions to another opportunity. Under the use of this simple mean, not only did the crusts become detached, but the eruption completely disappeared. In a few days after, the child became morose and languid; headach soon supervened, followed by fever, delirium and convulsive movements (meningitis). These symptoms were treated by the application of leeches behind the ears, of cold to the head, the use of calomel, &c. The affection became rapidly aggravated, and coma set in. One morning I found the child in a hopeless state: cadaveric paleness; the eyelids half closed; the eyes turned up; the extremities cold; the pulse thready; respiration feeble, with commencing râle; agony. Feeling the necessity of acting vigorously, and recollecting the source of the evil, I had the head immediately shaved and rubbed with a strong ointment of tartar emetic (two drachms of tartar emetic to an ounce of lard), at the same time that the legs were reddened by sinapisms, and a stimulating mixture was exhibited. The eruption was produced between the morning and the evening, and from that time the child appeared to revive. The pulse rose; warmth was restored; motion and intelligence reappeared; in a word, the patient began to convalesce.

I might have employed other means—say a blister to the head. I do not consider the happy issue of the case as the result of such a remedy, but as that of the revulsive and stimulant treatment in general. Is it by displacing the internal phlegmasia, by restoring the cutaneous eruption, or simply by raising the strength, that the stimulants succeeded? This much is certain, that life was flickering, and that it was necessary to rekindle it, even at the risk of aggravating the cerebral inflammation. No time was to be lost. I am far from assuming, alas! that the result will always be as favourable, for I have seen many poor children treated not less actively sink under this terrible malady. The preceding was an

example of what I should be inclined to call *nervous* agony. In the following case the *point de depart* was different.

CASE II.—Madame L. gave birth, in 1848, to twins, which were weakly, and often laboured under illness. During the early period of their existence, one of them was attacked, at the age of four months, with a pneumonia, which, in spite of every care, made rapid progress, so much so, that one day I found the wretched creature pale, cold, almost pulseless, scarcely breathing, in fact differing but little from a corpse. In despair I immediately applied a blister to the sternum, and administered every quarter of an hour a teaspoonful of a mixture consisting of a grain and a half of tartar emetic, dissolved in thirteen drachms of sugar and water. The little creature gradually revived, respiration was re-established, the pulse increased in strength, vomiting and discharges from the bowels took place. The child was saved. It now enjoys good health.

I doubted very much that this energetic treatment could be borne by so frail a creature, and yet to it it certainly owed its life. What was in this case the starting-point of the agony? The lung, undoubtedly; but the anemia was not less threatening than the asphyxia, so that the blister, and even the emetic seemed to me to have acted as much in the capacity of stimulants as of resolvents, at least if we may judge by the promptitude of the result. The following case is of the same nature:—

CASE III.—M. M., Professor at the Military Hospital, aged thirty-six years, of a nervous lymphatic constitution, subject to attacks of gout and asthma, was seized in 1840 with a severe pneumonia, which was treated by bleeding and large doses of tartar emetic. However, the disease went on; the patient was pale, debilitated, subject to fainting fits. One evening, on visiting him, I was struck with his hippocratic countenance, his cadaverous paleness. I was told that he had just fallen asleep. His hand was cold; his pulse vermicular; respiration scarcely perceptible; the patient did not answer to his name. Syncope was evident,—death was at hand. I immediately applied ammonia to the nostrils, and rubbed the face and limbs with vinegar and eau de Cologne. I sent for a large blister, which I applied to the chest, and a stimulating mixture of balm water, orange-flower water, tincture of cinnamon, and syrup of ether, which I administered in small quantities at a time. For long I was uncertain whether life or death should gain the ascendant. At length, at the end of half an hour, which to me appeared an age, heat was restored, the pulse rose, the chest expanded, the patient opened his eyes, and regained his consciousness. He felt so plainly that he had escaped death, that the first words he uttered were to thank me for having restored him to life, and he always retained the recollection of that critical moment, for he recovered then, but died lately of a chronic cerebral affection.

It is evident that had I not chanced to visit him, and had I not employed the active stimulants used to restore him to animation, he would have died. It appears that an acute affection of the chest

may induce death through failure of the circulation; but, in the primary affections of the circulating apparatus, the agony may result either directly from the impeded action of the heart, or indirectly from the influence of the heart upon the lungs.

CASE IV.—M. V., aged 60, of a broken-down constitution, was affected with chronic pulmonary catarrh, with secondary dilatation of the right cavities of the heart, giving rise to palpitations, irregularity of the pulse, attacks of asthma, and, lastly, serous infiltration of the lower extremities. In February, 1852, these symptoms became intense, the dyspnœa permanent, the œdema greater, and, in spite of the employment of appropriate treatment, cyanosis set in. One day I found him in a state of profound debility: his countenance hippocratic; his extremities cold; he had slight comatose delirium; his pulse was weak, unequal, and so slow that it did not beat forty in the minute. It might have been thought that such a state of depression would speedily terminate in death, and a repugnance might be felt to the use of painful remedies which would probably be useless. However, I had a blister applied to the chest, warmed the extremities, and administered a stimulating mixture (lime-flower water, tincture of cinnamon, and syrup of orange peel), *café noir*, and broth. The pulse, warmth, and strength were gradually, but slowly, restored, and the patient lived again. In this case it was through the circulation life was threatened; in the following, although the heart was primarily affected, it was by the lungs that life was near being extinguished.

CASE V.—In 1841, I, with two others, attended General C., labouring under disease of the heart with anasarca. One evening we found him in a state of confirmed agony, with loss of consciousness, his countenance distorted and livid, cold sweats, intense tracheal rattle, convulsive movements, thready pulse, &c. We all thought he had only some moments to live. Already one of my colleagues had withdrawn, when, to clear my conscience, I entreated the other to make some scarifications on the lower limbs, which were considerably infiltrated. He complied and we retired, having tendered our condolence to the family. What was my astonishment next morning to receive a message that the General wished to see me. I hastened to him, and found that he had completely rallied, retaining no recollection of the night, which he thought he had passed in a quiet sleep, this, I may parenthetically observe, ought to reassure us as to the torments which some persons appear to suffer in the agony, who, I am now convinced, are not sensible of any such suffering. But the punctures made had discharged so abundantly, that the patient's bed was soaked with the fluid, which streamed on the floor. The swelling of the limbs had gone down, and the nursetender informed me that the respiration had gradually become free, the pulse and warmth had returned, and intelligence had been restored. The patient, nevertheless, really died eight days after; still he had once rallied from a true agony.

Organic diseases of the heart sometimes give rise to rapid symp-

toms, with sanguineous turgescence, which demand another kind of interference; for example:—

CASE VI.—A man, aged 25, of strong sanguineous constitution, a gardener, came to the Clinique in 1837, affected with narrowing of the aortic orifice and general hypertrophy of the heart. He was subject to attacks of dyspnœa, with impending asphyxia, from which he was relieved by small bleedings and the use of revulsives. It happened that one of these attacks, more than ordinarily intense and obstinate, occurred with a profound feeling of anguish and terror: the face was livid and tumid; the pulse small and intermittent; the respiration stertorous; the extremities cold, so that the bystanders thought death both near and inevitable. Venesection and sinapisms having failed, I advised, in this extreme case, a remedy of energy proportioned to the imminence of the danger, and I caused the four limbs to be covered with cupping-glasses with scarifications, to the number of about one hundred. Soon, under the influence of this vast revulsion, the chest became free, the pulse recovered its usual regularity and fulness, and the patient was saved. Twice, at intervals of some weeks, I removed these formidable symptoms by the same means, but the patient sunk under a third attack, which came on in the night after an act of intemperance.

The cases of agony directly referrible to an idiopathic lesion of the lungs are, as I have said, more common than the preceding; such are the following:—

CASE VII.—M. S., aged 82, for many years subject to gouty and catarrhal attacks, frequently suffers from more or less rapid congestions of the lungs, produced under the influence of very different causes, but particularly of moral emotions, to which his irritable character renders him liable. One evening, during the winter of 1852, after an altercation at play, he was seized with dyspnœa, which in a few moments became so severe as to threaten immediate suffocation. On visiting him about an hour after the commencement of the attack, I found him sunk in a kind of coma, from which he occasionally roused, struggling and crying out, “I am smothering.” His face was pale, livid, wild looking, covered with perspiration; his breathing was painful, sibilant, and stertorous; the extremities were cold and clammy; the pulse was scarcely to be felt; the action of the heart was weak, slow, and irregular. I immediately had large sinapisms applied to the legs and arms, jars of hot water to the soles of the feet, a blister to the chest, and gave him alternately teaspoonfuls of balm tea, ether in *eau sucrée*, generous wine, &c. By degrees the heat returned; the pulse increased in strength; some efforts at coughing brought up mucous sputa; respiration became freer; the stertor disappeared; consciousness was restored,—but this favourable change required two hours’ laborious exertions; then excitement succeeded to torpor. A teaspoonful of syrup of acetate of morphia induced a quiet sleep, and the next day I found the patient

in his usual state. Some months afterwards he sank under anasarca, consequent on dilatation of the right side of the heart.

The following case presents much analogy to the foregoing, as to the phenomena of the agony:—

CASE VIII.—M. L., curé, aged 50, was affected with albuminuria, with slight hypertrophy of the heart, and some infiltration of the lower limbs. He also laboured under catarrhal dyspnœa, and was subject to pulmonary congestions, frequently accompanied by attacks of hemoptysis. During a year that he was under my care, I several times removed these latter symptoms by means of antimonials and revulsive remedies. About the middle of September, 1852, the catarrh and dyspnœa being intense, a little blood appeared in the expectoration. The ordinary measures were ineffective. The bronchial tubes rapidly became loaded; and one evening I found him in a state of extreme anxiety,—his countenance livid, wild-looking, streaming with perspiration; his lips blue, his extremities livid, cold, and moist; he had tracheal rattle; and his pulse was slow, feeble, and intermitting. Two ecclesiastics who were present, accustomed to attend the dying, considered that he was on the point of expiring. I applied sinapisms to the extremities and the sternal region, and administered a little balm tea. The asphyxia increasing, the cough being abortive and insufficient to remove the bronchial mucus, I gave him some spoonfuls of a generous white wine which he was fond of, at the same time covering the upper and lower extremities with dry cupping-glasses, to the number of more than eighty. Under the influence of the internal excitement, and of this powerful revulsion, the cough became stronger, the sputa were expelled, the chest was relieved, the skin grew warm, the pulse became firmer and more regular,—but the patient was a little agitated, owing, perhaps, to a slight degree of intoxication, although he had only taken about two ounces of wine. I then added thirty drops of laudanum to a wineglassful of sweetened balm tea, directing that he should have a teaspoonful of it every quarter of an hour. At last, after three hours of anxious and unremitting exertions, the patient fell quietly asleep. The next morning I found him calm, fully aware of the danger he had escaped, and thanking me warmly for having saved his life. The two clergymen were not a little astonished to find their colleague raised from the dead,—to use their own expression. There still remained, however, a slight febrile action and some trifling diarrhœa, which yielded to the application of twenty cupping-glasses, with scarifications, to the thighs, a ptisan of rice and gum, poppy lavers, and attention to diet. But the principal affection, the albuminuria, pursued its course, the anasarca increased, and the patient sank in a month after the circumstances above related.

I might add other examples to the foregoing, although these fortunate cases are greatly the exception, compared with the im-

mense proportion of patients who sink when they have once arrived at the degree of *agony* I have described. We see, however, that there is no situation so extreme as not to leave a ray of hope, and this is sufficient to authorize us to lay it down as a principle, never to abandon a patient until death has actually taken place. But it must be added, that such triumphs are only obtained by means of a strong faith in the power of our art, and of an activity and perseverance which nothing wearies or disheartens, and a presence of mind which nothing can disturb. Accordingly, it will be remembered, that almost all these successful cases occurred in private practice, where the patient is the object of an incessant and active solicitude. However, in addition to Case iv., I might bring forward some instances from my hospital practice. Thus I recollect, among others, a young girl in typhoid fever, over whose face I had thrown the winding sheet to conceal the spectacle of death from those about her, and whom I was not a little put out of countenance to find, at the next morning's visit, quite alive. It is for such reasons that I never omit, in going round the hospital, to prescribe active remedies for patients apparently in agony, directions which are generally received by the attendants with a smile of incredulity; but I represent these prescriptions *in extremis*, as a duty of conscience, a sort of sacred obligation of which it is sufficient if the utility be obtained in one case in a thousand, were that only in prolonging by some days, or even by some moments, the life of the sick. Let us remember, besides, that an instance of success of this kind is that which is best adapted to display the skill of the practitioner, as proving him to be armed with a power almost divine.

What is the use of tormenting poor dying people? is a question we hear repeated every day. In the first place, it is not proved that dying people are very sensible to pain; then we have just shown that these tortures may be good for something; lastly, ask the unhappy being who is suffocating, and who feels himself dying,—ask the weeping relatives who implore you to save him,—what they think of your sentimental philanthropy? But, beyond all these professional considerations, there is something positive, something glorious for science, and happy for humanity, in the consciousness, based on facts both numerous and authentic, that we may succeed.

In looking over the observations I have made, in reference to cases presenting considerable differences, it will be remarked that, after all, our resources are neither numerous nor various. With the exception of some abstractions of blood, or of serum (Cases v. and vi.), these resources consist in the adoption of the stimulant plan, either local or general, internal or external, direct or indirect. And the reason of this is evident, namely, that death resulting finally, from the cessation of the action of the principal organs, the problem consists, practically, in reanimating these exhausted organs, whatever may be the cause of their exhaustion. Here triumphs, in all its splendour, the doctrine of the elements. In fact, it is the

elements weakness, syncope, asphyxia, stupor, paralysis, which we must now combat, in turn or together.

Well, against all these elements it is almost always stimulation we must have recourse to, at the risk of momentarily aggravating the primitive, original element, whether that be inflammatory or otherwise, for, after all, the first condition necessary to being cured is to live, and the question is of retaining life, reserving that of curing for a subsequent period.

As to details I shall briefly explain them, with the assistance chiefly of the valuable labours of M. Piorry. This writer divides the treatment of the asphyxia of persons in the *agony* into the preventive and the curative. With the preventive treatment we have at present nothing to do, for we are treating specially of the confirmed *agony*. It will be sufficient then under the head of preventives to enumerate a warm and dry atmosphere, abstinence from drinks, nourishing diet, moderate exercise, and above all the treatment of the principal disease.

As to curative treatment, we find again set down the inspiration of a dry and warm air, as a mean of removing in the form of vapour a portion of the bronchial fluid; but this is only of slender utility, especially in urgent cases. It appears to me more essential to procure pure and frequently renewed air, to favour the oxygenation of the blood. Abstinence from drinks also is but a feeble auxiliary at the period of urgency I have supposed,—it is, moreover, rendered compulsory by the existence of difficulty of deglutition. Some stimulating mixture, wine, and broth, should be given in small quantities, frequently repeated, care being taken to prevent the liquids passing into the larynx, as was done in most of the cases described in this essay. It is essential to keep the head and trunk raised to favour expectoration in cases of pulmonary obstruction; but in cases of syncope the horizontal position is indispensable.

To assist the efforts at expiration, or rather to promote cough and expectoration, is the fundamental indication in cases of tracheal rôle. It is then recommended to induce the patient to cough and get up the mucus; but in the majority of instances strength is deficient, and intelligence has ceased. We should have *tussific* remedies, if I may be allowed the term, as we have sternutatories, emetics, &c. Expectorant is not a synonymous term;—we excite, keep up, and strengthen the cough, and thus the expectoration, on the one hand, by stimulating the general strength by means of aromatic waters, wine, and spirituous tinctures; on the other, by slightly irritating the larynx by means of inhalations, ever so little irritating, acetic, ammoniacal, or other.

General bleeding is rarely indicated, except in cases of manifest congestion. There is danger of its extinguishing the failing powers; so that it may be useful, while we empty the vessels, to insist on the use of stimulants. Local bleeding is less weakening; it is particularly useful combined with derivation, a double effect produced by

cupping-glasses with scarifications, boldly and extensively applied, as in my sixth case.

Without abstracting the blood itself, we may endeavour to deprive it of part of its serosity. On this subject M. Piorry passes in review sudorifics, diuretics, sialogogues, all means on which he justly sets but little value; but he does not, in my opinion, render sufficient justice to emetics as stimulants, and particularly as means of expelling the bronchial mucus; he prefers purgatives, the action of which is too slow, when death is imminent.

In almost every case of obstructed respiration, and consequently of râle, we are led by instinct to apply blisters on the chest, less, certainly, with a view to get rid of the serum, than as a revulsive and general stimulant. With blisters we naturally class sinapisms, and, above all, dry cupping, that powerful agent of combined stimulation and derivation; then stimulating frictions, whether warm, dry, or moist.

We must not forget, as direct modes of unloading parts in various dropsies (Case v.), scarifications, acupuncture, and thoracic or abdominal paracentesis.

Fixed tonics are only accessorially indicated, for in the urgent cases I have supposed we should prefer diffusible stimulants, the action of which is instantaneous; but as adjuvants tonics are rationally indicated.

Opiates should be exhibited when there is acute pain, convulsions, agitation, nervous hyper-excitement, circumstances which are not rare before and after the *agony* (Cases VII. and VIII.) I think I have saved life by means of opium in patients on the point of sinking under the violent pains and convulsions of cerebro-spinal meningitis; but when life is on the point of being extinguished by syncope or by asphyxia, when sensibility and motion are wanting, it is evident that narcotism is contra-indicated,

Finally, inflating the lungs, pumping out the bronchial mucosities by means of a sucking syringe, or tracheotomy, may be found applicable in exceptional cases.

It will be seen that these numerous means are not all equally important, and that it is easy to reduce them to a very small number in reference to the simplicity of the indications. To recapitulate—

The *agony* is not always followed by death.

It admits of a special treatment, like the other phases of disease, just as a distinct disease.

We ought to endeavour to combat the *agony* in all cases, as we cannot tell those in which assistance will be absolutely inefficacious.

The *agony* consists in the diminution or arrest of function of one or more of the organs or systems essential to life. These systems are principally those of innervation, circulation, and respiration.

The *agony* is variable in its forms and degrees; but its several

aspects have this in common, that they appear to portend approaching death.

The curative indication consists essentially in reanimating the functions which are failing.

The means of restoring those in *agony* to life vary but little, whatever be the organ most seriously affected.

These means are almost all comprised under the head of stimulant medication, whether direct or indirect.

It is essential to act with vigour and perseverance, modifying our proceedings according to the indications and the effects observed.

Among the indications, that of re-exciting sensibility occupies the first place, for it is equally applicable to failure of innervation, circulation, and respiration, paralysis, syncope, and asphyxia.

Slow asphyxia, by passive pulmonary engorgement, is the most common, and often the most serious form of *agony*; it may be produced primarily or secondarily.

To re-establish respiration by unloading the bronchi is therefore the most ordinary indication. We accomplish this by stimulating the general strength, and then by exciting the cough, the double element on which expectoration depends.

It is futile to remove the mucus from the bronchi, and excite the cough, if we do not restore to the patient the strength necessary to induce and keep up expectoration.

It is to this end that we apply energetically and unremittingly internal or external stimulants, especially aromatics, wine, spirits, in small and repeated doses, concurrently with blisters to the chest, sinapisms, stimulating frictions, dry cupping, or cupping with scarifications over the entire surface of the extremities and trunk, &c.

The limits of our efforts is marked by a single phenomenon, the cessation of the pulsations of the heart.

If it is true that these efforts are in an hundred or a thousand instances unproductive of any satisfactory result, it is sufficient that their efficacy is demonstrated by some cases which are less rare than is generally supposed to make it right that the practitioner should feel it his duty to adopt them in all cases.—*Bulletin Général de Thérapeutique*, 1853, pp. 97 and 201.

On Urate of Lime in the Coats of the Veins, in Cases of Gouty Concretions. By J. L. C. SCHROEDER VAN DER KOLK.

It is generally known that in cases of gout in which concretions form, the uric acid is not entirely removed from the system by the kidneys, but that it accumulates as urate of lime in various parts of the body, especially in the fingers. On examining, after death, the hands of a patient in whom these concretions existed to a great degree, I not only found the tendons of the flexors and extensors of the fingers, as well as the ligaments, deeply coated with urate of lime, but also discovered this salt forming tolerably large knobs un-

der the very skin, so that some of the digital nerves were here and there completely surrounded and perforated by it. But my attention was particularly attracted by observing, after I had dried a portion of the skin in which the arteries and veins had been injected with red and blue, that the latter vessels existed as white ramifications, in consequence of the great quantity of urate of lime which had been deposited in their coats, while the arteries were quite free from any such change. The valves of the veins, too, appeared to have been injured or altered by a deposition of the same salt. Thus, we can scarcely ever succeed in injecting the ramifications of the veins, at least those of the fingers, from the trunk; but in this case, I saw to my surprise, that the veins, especially of the hand, and, in a less degree, those of the fingers, became most finely filled with the blue matter, which was injected into the veins of the forearm.

I am not aware whether this alteration of the veins in gout has been described by any writer. That it is closely connected with absorption appeared to me evident from this, that in the situations where the skin was most penetrated with the urate of lime, the cutaneous and capillary veins were most abundantly studded with the salt in their interior. It thus appears that the urate of lime, having been separated from the blood, the tendons, and the skin of the hand, is in part taken up again by the veins, and so becomes more widely dispersed through the system, while part is at the same time deposited also in the coats of the minutest capillaries, and especially as the valves are simultaneously destroyed, cannot be altogether without influence on the circulation of the blood.

The violent pain by which this patient had so often been tormented can be easily explained, as the urate of lime had at the same time been deposited around the nerves, and had even penetrated the latter, as was very plainly demonstrable in the nerves of the thumb and index finger, which I examined as to this point, and which appeared somewhat swollen in consequence. It must be left to further investigations to show how far the pain in gout is always to be ascribed to irritation or pricking of the nerves of the great toe in consequence of the deposition of urate of lime between the sheaths of the nerves.—*Nederlandsch Lancet*, July and August, 1853, p. 97.

On Primary Cancer of the Spleen. By DR. GUENSBURG.

THE spleen is one of the organs least liable to be primarily affected with cancer. Lebert, in his work on Cancerous Diseases, states that he has never met with such a case. The exceptional nature of the lesion, therefore, renders the following history worthy the attention of pathological anatomists.

A woman, aged 40, had suffered for a year from weakness, attacks of fever, and lancinating pains in both hypochondriac regions, so violent as to deprive her of sleep. Dr. Guensburg having been

summoned to attend her, found her in a state of extreme emaciation. In the left hypochondrium was a tumour reaching to within half an inch of the epigastrium, and extending downwards to the level of a line drawn horizontally from the umbilicus. This tumour was hard, and presented an irregular surface; the dulness on percussion extended to the left axilla; the liver retained its ordinary volume; there was constipation; the sounds of the heart were normal; the pulse was small, and varied from 100 to 108; the blood contained a few white globules. The patient died with symptoms of general dropsy in four months after having been placed under treatment. On *post-mortem* examination there was found effusion of limpid serum in both pleura; some tubercles in the apices of both lungs; limpid serum in the pericardium. The heart was small; there was some opacity of the mitral valve; the heart and great vessels contained a little blood and some fibrinous coagula. There was effusion of serum into the abdomen. The spleen was about a foot in length, six inches in breadth, and the same in thickness; its surface was studded with hemispherical elevations as hard as cartilage. The tumours, which were difficult to remove, were of a deep brown colour mixed with white, and occupied the entire spleen; there were scarcely any traces of normal tissue in the organ. The lymphatic glands surrounding the spleen and pancreas were infiltrated with a soft whitish substance. The peritoneum was swollen, opaque, and easily torn. The remaining abdominal organs presented nothing remarkable. Microscopic examination revealed the existence of cancer cells in the morbid tissue of the spleen.—*Gazzetta Medica Italiana Toscana*, 1854, p. 38, from *Guensburg's Zeitschrift*, IV. 5, 1853.

INDEX

TO THE SEVENTEENTH VOLUME.

The Letters D. P. S. refer to the Reports of the Dublin Pathological Society ; D. O. S. to those of the Dublin Obstetrical Society ; and T. C. P. to the Transactions of the Association of the King and Queen's College of Physicians.

- ABEL and Bloxam's hand-book of chemistry, *Rev.*, 166.
- Abscess in the brain, Mr. Hamilton on, D. P. S., 220 ; Dr. Hutton on, D. P. S., 441.
- Adams, Mr., anatomy, diseases of the prostate gland by, *Notice*, vii.
- Agony, Professor Forget on the treatment of the, *Translation*, 490.
- Analysis, Professor Wöhler's hand-book of inorganic, *Notice*, vii.
- Anatomy, the microscope in its application to vegetable, by Dr. Schacht, *Rev.*, 201 ; Mr. MacLise on surgical, *Rev.*, 217 ; of the prostate gland, Mr. Adams on, *Notice*, vii. ; Siebold and Stan-
nius' comparative, *Notice*, xvii.
- Aneurism, of the aorta opening into the pericardium, Dr. Neligan on, D. P. S., 224 ; Dr. Gordon's case of, D. P. S., 229 ; at the base of the brain, Dr. Mayne on, D. P. S., 451.
- Anus, Mr. Ashton on diseases of the, *Rev.*, 409.
- Aorta, Dr. Stokes on the diseases of the, *Rev.*, 120 ; Dr. Neligan on aneurism of, D. P. S., 224 ; Dr. Gordon's case of aneurism of the, D. P. S., 229.
- Aortic valves, Dr. M'Dowel's case of disease of the, D. P. S., 229.
- Appareil immobile, Mr. Gamgee on, in the treatment of fractures, *Rev.*, 161.
- Arthritis, Professor Smith on chronic rheumatic, of the wrist joint, D. P. S., 223 ; of the hip joint, D. P. S., 440.
- Ashton, Mr., on diseases of the rectum, *Rev.*, 409.
- Atony, R. Virchow on, *Trans.*, 248.
- Atthill, Dr., on chloroform in puerperal convulsions, D. O. S., 453 ; in midwifery, D. O. S., 462.
- Auriculo-ventricular opening, Dr. Banks' case of contraction of the left, D. P. S., 226.
- Auscultation, Drs. Hughes and Bennett on, *Rev.*, 365.
- Babington, Dr., on a case of poisoning by laburnum seeds, 246.
- Banks, Professor, on ovarian disease, D. P. S., 219 ; case of contraction of the left auriculo-ventricular opening by, D. P. S., 226 ; on cirrhosis of the liver, D. P. S., 231 ; clinical reports by, 318 ; on pneumothorax, 318 ; on emphysema, D. P. S., 450.
- Barlow, Mr., on fatty degeneration, *Rev.*, 399.
- Beatty, Dr., on chloroform in midwifery, 355 ; on cancer of the uterus, D. P. S., 443 ; on polypus of the uterus, D. P. S., 450.

- Becquerel and Rodier's pathological chemistry, *Rev.*, 390.
- Bed, Mr. Young on a new surgical and invalid, 111.
- Bennett, Dr., on pulmonary tuberculosis, *Rev.*, 168; lectures on clinical medicine by, *Rev.*, 365.
- Bird, Dr. Golding, elements of natural philosophy by, *Notice*, xviii.
- Blood, analysis of, 391; free fat in, 467.
- Bloxam and Abel's chemistry, *Rev.*, 166.
- Bone, Dr. Hutton, on caries of the temporal, D. P. S., 441.
- Brady, Dr., case of ulceration of the intestines by, D. P. S., 227.
- Brady, Dr. James, on scarlatina occurring a few days after delivery, D. O. S., 459.
- Brain, plates of the, by Mr. Swan, *Rev.*, 190; Mr. Hamilton on abscess in the, D. P. S., 220; Professor Gellerstedt on cysticercus cellulosæ in the, *Trans.*, 253; Dr. Hutton on abscess in the, D. P. S., 441; aneurism at the base of the, Dr. Mayneon, D. P. S., 451.
- Bronchitis, Dr. Griffin on, accompanying whooping-cough, 304.
- Butcher, Mr., on some rare injuries of joints, 20; on hydrocele, 326.
- Calculi, consisting of Heller's urostealith, Dr. Moore on, 473.
- Cancer of the uterus, Dr. Beatty on, D. P. S., 443; of the stomach, Dr. Lees on, D. P. S., 445; of the spleen, Dr. Guensburg on primary, *Trans.*, 503.
- Cancerous affections of the os uteri, Dr. Johns on, 99.
- Caries, of the cervical vertebræ, Professor Smith on, D. P. S., 438; of the temporal bone, Dr. Hutton on, D. P. S., 441.
- Cauliflower excrescence of the os uteri, Dr. Johns on, 99.
- Chemistry, Abel and Bloxam's hand-book of, *Rev.*, 166; Dr. Gregory's hand-book of inorganic, *Notice*, v.; Dr. Wittstein's practical, *Notice*, v.; Becquerel and Rodier's pathological, *Rev.*, 390.
- Child, Dr., on indigestion, *Notice*, xvii.
- Childbed, Dr. Churchill, on paralysis occurring in, 257.
- Children, Dr. Henriette on peritonitis in, *Trans.*, 250.
- Chloroform, M. Robert on the administration of, *Trans.*, 247; Dr. Beatty on, in parturition, 355; Dr. Atthill on, in puerperal convulsions, D. O. S., 453; Dr. O'Reilly on the local application of, D. O. S., 457; in midwifery, Dr. Atthill on, D. O. S., 462.
- Cholera, the common sense of, by a practical practitioner, *Rev.*, 202.
- Chordæ tendineæ, Dr. Gordon on rupture of the, D. P. S., 445.
- Churchill, Dr., on an abortion tenaculum, D. O. S., 235; on a new form of pessary, D. O. S., 235; on paralysis occurring during gestation and in childbed, 257.
- Cirrhosis of the liver, Dr. Gordon on, 345; D. P. S., 225; Dr. Banks' case of, D. P. S., 231.
- Cock, Dr., manual of obstetrics by, *Rev.*, 160.
- Colles, the late Professor, remarks on the diaphoretic treatment of tetanus, 73; selections from the unpublished manuscripts of, 82; on a peculiar disease of the rectum, 82.
- Comparative anatomy, Siebold and Stannius', *Notice*, xvii.
- Concretion, pulmonary, Mr. Hamilton on, D. P. S., 446.
- Concretions, gouty, Dr. Schroeder van der Kolk on urate of lime in the coats of the veins in cases of, *Trans.*, 502.
- Conjunctivitis, Dr. Griffin on chronic, 310.
- Consumption, Dr. Thompson's lectures on, *Rev.*, 168; Dr. Bennett on, *Rev.*, 168.
- Convulsions, Dr. Atthill on chloroform in puerperal, D. O. S., 453.
- Corrigan, Dr., case of a foreign substance passed from the intestines, D. P. S., 228.
- Cretinism, Dr. Guggenbühl on, *Notice*, v.
- Crumpe, Dr., case of tumour of labium pudendi by, 466.

- Cysticercus cellulosæ in the brain, Professor Gellerstedt on, *Trans.*, 253.
- Degeneration, fatty, Mr. Barlow on, *Rev.*, 399.
- Delivery, Dr. Brady on scarlatina occurring a few days after, D. O. S., 459.
- Dendy, Mr., on the varieties of pock, *Notice*, xviii.
- Diabetes mellitus, Sir H. Marsh on the treatment of, 1.
- Diaphoretic treatment, value of, in tetanus, 73.
- Diseases of the heart and the aorta, Dr. Stokes on the, *Rev.*, 120.
- Dislocation of the metatarsus downwards and backwards, Mr. Tufnel on, 65; Mr. Smyly on, 317; of the upper extremity of the radius forwards, Mr. Tagert on, 296; of the spine, Dr. Hutton on, D. P. S., 449.
- Douche, Dr. Sinclair on the induction of premature labour by means of the, D. O. S., 240; on a new continuous, D. O. S., 242.
- Dreaming, Dr. Popham on, *Rev.*, 414.
- Druitt, Mr., surgeon's vade-mecum by, *Notice*, vi.
- Darand Fardel, M., on the diseases of old persons, *Rev.*, 415.
- Emphysema, Dr. Banks on, D. P. S., 450.
- Empyema, Dr. Gordon on, 336.
- England, Professor Santesson on the medical schools of, *Rev.*, 146, 382.
- Epileptic fits during pregnancy, Dr. Griffin on, 315.
- Erichsen, Professor, science and art of surgery, *Rev.*, 425.
- Fat, free, in the blood, 467.
- Fatty degeneration, Mr. Barlow on, *Rev.*, 399; of the liver, 467.
- Flax-mills, Dr. Moore on injuries from, 60.
- Fleming, Dr., on a pendulous tumour removed from the labium, D. P. S., 225; on ulceration of the larynx, D. P. S., 442.
- Foreign substance passed from the intestines, Dr. Corrigan's case of, D. P. S., 228.
- Forget, Professor, on the treatment of the agony, *Trans.*, 490.
- Fractures, Mr. Gamgee on the starched apparatus in the treatment of, *Rev.*, 161; Dr. Middeldorff on, *Rev.*, 396.
- France, Professor Santesson on the medical schools of, *Rev.*, 146.
- Gall bladder, ulceration of, Dr. Lees on, D. P. S., 447.
- Galvanic current, Dr. Todd's experiments on the, *Notice*, vii.
- Gamberini, Dr. Pietro, on the truces and cure of venereal affections, *Trans.*, 476.
- Gamgee, Mr., on the starched apparatus in the treatment of, *Rev.*, 161.
- Gastritis, Dr. Griffin on simulated chronic, 308.
- Gellerstedt, Professor, on cysticercus cellulosæ in the brain, *Trans.*, 253.
- Gestation, Dr. Churchill on paralysis occurring during, 257.
- Gland, Mr. Adams on the prostate, *Notice*, vii.
- Gordon, Dr., on cirrhosis of the liver, 345; D. P. S., 225; on aneurism of the aorta opening into the pericardium, D. P. S., 229; clinical reports by, 336; on empyema, 336; on pleuritis, 340; on rupture of the chordæ tendinæ, D. P. S., 445; on ulceration of the trachea and intestines, D. P. S., 447.
- Gout and its complications, Mr. T. Spencer Wells on, *Rev.*, 417.
- Gouty concretions, Dr. Schroeder van der Kolk on urate of lime in the coats of the veins in cases of, *Trans.*, 502.
- Gregory, Dr., hand-book of inorganic chemistry, *Notice*, v.
- Griffin, Dr., on treatment as a test of certain pathological conditions, 300; on bronchitis with whooping-cough, 304; on pneumonia with typhoid symptoms, 306; on simu-

- lated chronic gastritis, 308; on conjunctivitis, 310; on iritis, 313; on epileptic fits during pregnancy, 315.
- Guensburg, Dr., on primary cancer of the spleen, *Trans.*, 503.
- Guggenbühl, Dr., on cretinism, *Notice*, v.
- Hamilton, Mr., on abscess in the brain, D. P. S., 220; case of scrofulous disease of the knee joint by, D. P. S., 230; on pulmonary concretion, D. P. S., 446.
- Head, Professor Smith on sanguineous tumour of the bones of the, D. P. S., 451.
- Heart, Dr. McDowel on diseases of the, 90; effects of dilatation of the right side of the, 90; Dr. Stokes on the diseases of the, *Rev.*, 120.
- Heller's urostealith, calculi consisting of, 473.
- Henriette, Dr., on peritonitis in children, *Trans.*, 250.
- Hip joint, Professor Smith on chronic rheumatic arthritis of the, D. P. S., 440.
- Holland, Professor Santesson on the medical schools of, *Rev.*, 146.
- Holland, Dr., on the Irish school of medicine, *Notice*, vi.
- Hooping-cough, Dr. Griffin on bronchitis accompanying, 304.
- Hospitals of Paris and London, Dr. Landi on, *Rev.*, 146.
- Hughes, Dr., on auscultation, *Rev.*, 365.
- Hutton, Dr., on caries of the temporal bone, D. P. S., 441; on dislocation of the spine, D. P. S., 449.
- Hydrocele, Mr. Butcher on, 326.
- Hygiene, M. Tardieu's dictionary of, *Rev.*, 437.
- Iconographie ophthalmologique, by Dr. Sichel, *Rev.*, 218.
- Indigestion, Dr. Child on, *Notice*, xvii.
- Injuries, rare, of joints, Mr. Butcher on, 20; from flax-mills, Dr. Moore on, 60.
- Intestines, Dr. Brady's case of ulceration of the, D. P. S., 227; Dr. Corrigan's case of a foreign substance passed from the, D. P. S., 228; Dr. Gordon on the ulceration of the, D. P. S., 447.
- Inversio uteri, Dr. Johnston on, occurring during labour, D. O. S., 235.
- Iodine, injections of, in hydrocele, Mr. Butcher on, 326.
- Iritis, Dr. Griffin on, 313.
- Irritation, Dr. Law on spinal, 48.
- Italy, Professor Santesson on the medical schools of, *Rev.*, 146.
- Johns, Dr., on cancerous affections of the os uteri, 99.
- Johnson, Dr. G., on the relation between therapeutics and pathology, *Notice*, vi.
- Johnston, Dr. G., on inversio uteri occurring during labour, D. O. S., 235.
- Joints, Mr. Butcher on rare injuries of, 20; Mr. Gamgee on the starched apparatus in the treatment of diseases, *Rev.*, 161.
- Kearns, Mr., case of difficult labour by, 471.
- Kennedy, Dr. H., on pneumonia, 348.
- Knee joint, Mr. Hamilton's case of scrofulous disease of the, D. P. S., 230.
- Kolk, Schroeder van der, on urate of lime in the coats of the veins, *Trans.*, 502.
- Labium pudendi, Dr. Fleming on a pendulous tumour removed from the, D. P. S., 225; Dr. M'Clintock on a fibrous tumour removed from the right, D. O. S., 234; Dr. Crumpe's case of tumour of the, 466.
- Labour, Dr. Johnston on inversio uteri occurring during, D. O. S., 235; Dr. Sinclair on the induction of premature, D. O. S., 240; Mr. Kearns' case of difficult, 471.
- Laburnum seeds, Dr. Babington on a case of poisoning by, 246.

- Landi, Dr., on the hospitals, &c., of Paris and London, *Rev.*, 146.
- Larynx, Dr. Fleming on ulceration of the, D. P. S., 442.
- Law, Dr., on spinal irritation, 48.
- Lectures, Dr. Bennet's, on clinical medicine, *Rev.*, 365.
- Lees, Dr. on cancer of the stomach, D. P. S., 445; on scirrhus of the pancreas and liver, D. P. S., 447; on tubercular meningitis, D. P. S., 448.
- Lexicon, Dr. Mayne's expository, *Rev.*, 190.
- Little, Dr. on some uncommon forms of tetanus, 70.
- Liver, Dr. Gordon on cirrhosis of the, 345, D. P. S., 225; Dr. Banks' case, D. P. S., 231; Dr. Lees on scirrhus of the, D. P. S., 447; Dr. Popham's case of fatty degeneration of, 467.
- London, Dr. Landi on the hospitals of, *Rev.*, 146; Transactions of the Pathological Society of, *Notice*, v.
- Lungs, Professor Zenker on sarcinæ in the lungs, *Trans.*, 249.
- Luxation, congenital, of the radius, Professor Smith on, D. P. S., 452.
- M'Clintock, Dr., on a fibrous tumour removed from the right labium, D. O. S., 234; on a case of vaginal polypus, D. O. S., 234.
- M'Dowel, Dr., on diseases of the heart, 90; case of disease of the aortic valves by, D. P. S., 229.
- M'Swiney, Dr., on a case of retention of urine inducing miscarriage, D. O. S., 461.
- Maclise, Mr., on surgical anatomy, *Rev.*, 217.
- Marsh, Sir H., on the treatment of diabetes mellitus, 1.
- Massey, Dr., on the examination of recruits, *Rev.*, 387.
- Materia medica and therapeutics, Dr. Pereira on, *Rev.*, 144.
- Mayne, Dr., on aneurism at the base of the brain, 451.
- Mayne, Dr., expository lexicon of medical and scientific terms by, *Rev.*, 190.
- Medical life, reminiscences of, by Mr. Toogood, *Notice*, vii.
- Medical schools of France, England, Italy, and Holland, Professor Santesson on, *Rev.*, 146.
- Medicine, the Irish school of, Dr. Holland on, *Notice*, vi.; Dr. Bennett's lectures on clinical, *Rev.*, 365.
- Medicines, their uses and mode of administration, by Dr. Neligan, *Notice*, xix.
- Meningitis, Dr. Lees on tubercular, D. P. S., 448.
- Metatarsus, Mr. Tufnell on dislocation of the, downwards and backwards, 65; Mr. Smyly on, 317.
- Microscope, Dr. Schacht on the application of, to vegetable anatomy, *Rev.*, 201.
- Middeldorf, Dr., on fractures, *Rev.*, 396.
- Midwifery, Dr. Beatty's contribution, 355; Dr. Atthill on chloroform in, D. O. S., 462.
- Mills, flax, Dr. Moore on injuries from, 60.
- Miscarriage induced by retention of urine, Dr. M'Swiney's case of, D. O. S., 461.
- Moore, Dr. W., on injuries from flax-mills, 60.
- Moore, Dr. W. D., observations on urine and urinary calculi, 473.
- Natural philosophy, Dr. Golding Bird's elements of, *Notice*, xviii.
- Navy, statistics of the health of the, *Notice*, xviii.
- Neck, Professor Smith's case of glandular tumour of the, D. P. S., 232.
- Neligan, Dr., on aneurism of the aorta, D. P. S., 224; Medicines: their uses and mode of administration, by, *Notice*, xix.
- Nervous system, Mr. Swan on the, *Rev.*, 190.
- Obstetrical Society, proceedings of the Dublin, 234, 453.
- Obstetrics, Dr. Cock's manual of, *Rev.*, 160.
- Old persons, M. Durand Fardel on the diseases of, *Rev.*, 415.

- Ophthalmological iconography, by Dr. Sichel, *Rev.*, 218.
- O'Reilly, Dr., on the local application of chloroform, D. O. S., 457.
- Ossuteri, Dr. Johns on cancerous affections of the, 99.
- Ovarian disease, case of, by Professor Banks, D. P. S., 219.
- Pancreas and liver, Dr. Lees on scirrhus of the, D. P. S., 447.
- Paralysis, Dr. Churchill on, occurring during gestation, and in child-bed, 257.
- Paralyzed limbs, Dr. Todd's experiments of the effects of the galvanic current on, *Notice*, vii.
- Paris, Dr. Landi on the hospitals of, *Rev.*, 146.
- Parker, Mr. Langston, on the modern treatment of syphilitic diseases, *Notice*, xvii.
- Parturition, Dr. Beatty on chloroform in, 355; Dr. Atthill on the same, D. O. S., 462.
- Pathological chemistry, Becquerel and Rodier's, *Rev.*, 390.
- Pathological Society, proceedings of the Dublin, 219, 438; of the London, *Notice*, v.
- Pathology, Dr. Johnson on the relation between therapeutics and, *Notice*, vi.
- Pathology of stricture, Dr. Thompson on, *Rev.*, 203.
- Pereira, Dr., on materia medica and therapeutics, *Rev.*, 144.
- Pericardium, Dr. Neligan's case of aneurism of the aorta opening into the, D. P. S., 224; Dr. Gordon's case, D. P. S., 229.
- Peritonitis, Dr. Henriette on acute in children, *Trans.*, 250; Dr. Popham's case of chronic, 467.
- Pessary, Dr. Churchill on a new form of, D. O. S., 235.
- Philosophy, natural, Dr. G. Bird's elements of, *Notice*, xviii.
- Phthisis, Drs. Thompson and Bennett on, *Rev.*, 168.
- Pleuritis, Dr. Gordon on, 340.
- Pneumonia, Dr. Griffin on, with typhoid symptoms, 306; Dr. H. Kennedy on, 348.
- Pneumothorax, Professor Banks on, 318; Dr. Gordon on, 340.
- Pock, Mr. Dendy on the varieties of, *Notice*, xviii.
- Poisoning, Dr. Babington on a case of, by laburnum seeds, 246.
- Polypus, Dr. M'Clintock on vaginal, D. O. S., 234; of the uterus, Dr. Beatty on, D. P. S., 450.
- Popham, Dr., on sleep and dreaming, *Rev.*, 414; case of chronic peritonitis, by, 467.
- Practitioner, a practical, on the common sense of cholera, *Rev.*, 202.
- Pregnancy, Dr. Churchill on paralysis occurring during, 257; Dr. Griffin on epileptic fits during, 315.
- Proceedings of the Pathological Society of Dublin, 219, 438; of the Obstetrical, 234, 453.
- Prostate gland, Mr. Adams on the, *Notice*, vii.
- Pulmonary concretion, Mr. Hamilton on, D. P. S., 446.
- Radius, Mr. Tagert on dislocation forwards of the upper extremity of the, 296; Professor Smith on congenital luxation of the, D. P. S., 452.
- Recruits, Dr. Massey on the examination of, *Rev.*, 387.
- Rectum, the late Professor Colles on a peculiar disease of the, 82; Mr. Ashton on diseases of the, *Rev.*, 409.
- Regurgitation, tricuspid, Doctor M'Dowel on, 90.
- Reports, clinical, by Prof. Banks, 318; by Dr. Gordon, 336.
- Robert, M., on the administration of chloroform, *Trans.*, 247.
- Rodier and Becquerel's pathological chemistry, *Rev.*, 390.
- Santesson, Professor, on the medical schools of England, France, Holland, and Italy, *Rev.*, 146, 382.
- Sarcinæ in the lungs, Professor Zenker on, *Trans.*, 249.
- Scarlatina, Dr. Brady on, occurring a few days after delivery, D. O. S., 459.

- Schacht, Dr., on the application of the microscope to vegetable anatomy, *Rev.*, 201.
- Schools, medical, of England, Holland, France, and Italy, Professor Santesson on, *Rev.*, 146, 382.
- Schroeder van der Kolk, on urate of lime in the coats of the veins, *Trans.*, 502.
- Scirrhus of the pancreas and liver, Dr. Lees on, *D. P. S.*, 447.
- Scrofulous disease of the knee joint, Mr. Hamilton's case of, *D. P. S.*, 230.
- Sichel, Dr., "iconographie ophthalmologique" by, *Rev.*, 218.
- Siebold and Stannius, comparative anatomy by, *Notice*, xvii.
- Sinclair, Dr., on induction of premature labour, *D. O. S.*, 240; on a new continuous douche, *D. O. S.*, 242.
- Sleep and dreaming, Dr. Popham on, *Rev.*, 414.
- Smith, Professor, on chronic rheumatic arthritis of the wrist, *D. P. S.*, 223; of the hip joint, *D. P. S.*, 440; on a glandular tumour of the neck, *D. P. S.*, 232; on caries of the cervical vertebrae, *D. P. S.*, 438; on sanguineous tumour of the bones of the head, *D. P. S.*, 451; on congenital luxation of the radius, *D. P. S.*, 452.
- Smyly, Mr., on dislocation of the metatarsus, 317.
- Society, proceedings of the Pathological, 219, 438; of the Obstetrical, 234, 453; Transactions of the London Pathological, *Notice*, v.
- Spinal irritation, Dr. Law on, 48.
- Spine, Dr. Hutton on dislocation of the, *D. P. S.*, 449.
- Spleen, primary cancer of, Dr. Guensburg on, *Trans.*, 503.
- Starched apparatus, Mr. Gamgee on, in the treatment of fractures and diseases of the joints, *Rev.*, 161.
- Statistics of the health of the navy, *Notice*, xviii.
- Stokes, Dr., on the diseases of the heart and the aorta, *Rev.*, 120.
- Stomach, Dr. Lees on cancer of the, *D. P. S.*, 445.
- Stricture of the urethra, Dr. Thompson on the pathology of, *Rev.*, 203.
- Surgeon's vade mecum, by Mr. Druiitt, *Notice*, vi.
- Surgery, Professor Erichsen's science and art of, *Rev.*, 425.
- Surgical anatomy, Mr. MacLise on, *Rev.*, 217.
- Surgical bed, Mr. Young on a new, 111.
- Swan, Mr., plates of the brain by, *Rev.*, 190.
- Syphilitic diseases, Mr. Parker on the modern treatment of, *Notice*, xvii.; the truces of, Dr. Gamberini on, *Trans.*, 476.
- Tables of cases of the administration of chloroform in midwifery, by Dr. Atthill, 464.
- Tagert, Mr., on dislocation of the radius, 296.
- Tardieu, M., dictionary of hygiene, *Rev.*, 437.
- Tenaculum, Dr. Churchill, on an abortion, *D. O. S.*, 235.
- Tetanus, Dr. Little on some uncommon forms of, 70; Professor Colles' remarks on the diaphoretic treatment of, 73.
- Therapeutics, Dr. Pereira on, *Rev.*, 144; Dr. Johnson on the relation between pathology and, *Notice*, vi.
- Thompson, Dr., on pulmonary consumption, *Rev.*, 168.
- Thompson, Dr. Henry, on the pathology of stricture, *Rev.*, 203.
- Todd, Dr. R. B., experiments on the effects of the galvanic current by, *Notice*, vii.
- Tone and atony, R. Virchow on, *Trans.*, 248.
- Toogood, Mr., reminiscences of medical life by, *Notice*, vii.
- Trachea, Dr. Gordon on ulceration of the, *D. P. S.*, 447.
- Transactions of the London pathological Society, *Notice*, v.
- Treatment as a test of certain pathological conditions, Dr. Griffin on, 300.
- Tricuspid regurgitation, Doctor M'Dowel on, 90.
- Truces of venereal affections, Dr. Gamberini on, 476.

- Tubercular meningitis, Dr. Lees on, D. P. S., 448.
- Tuberculosis, Drs. Thompson and Bennett on pulmonary, *Rev.*, 168.
- Tufnell, Mr., on dislocation of the metatarsus, 65.
- Tumour, Dr. Fleming on a pendulous, removed from the labium, D. P. S., 225; glandular of the neck, Professor Smith's case of, D. P. S., 232; fibrous, of the right labium, Dr. M'Clintock on, D. O. S., 234; sanguineous, of the bones of the head, Professor Smith on, D. P. S., 451; of the labium pudendi, Dr. Crumpe's case of, 466.
- Ulceration of the intestines, Dr. Brady's case of, D. P. S., 227; of the larynx, Dr. Fleming on; D. P. S., 442; of the trachea and intestines, Dr. Gordon on, D. P. S., 447; of the gall bladder, Dr. Lees on, D. P. S., 447.
- Urate of lime in the coats of the veins, Dr. Schroeder van der Kolk on, *Trans.*, 502.
- Urethra, Dr. Thompson on stricture of the, *Rev.*, 203.
- Urine, Dr. M'Swiney's case of miscarriage induced by retention of, D. O. S., 461; and urinary calculi, Dr. Moore on, 473.
- Urostealith, calculi consisting of, 473.
- Uterus, Dr. Beatty on cancer of the, D. P. S., 443; on polypus of the, D. P. S., 450.
- Vade mecum, the surgeon's, by Mr. Druitt, *Notice*, vi.
- Vaginal polypus, Dr. M'Clintock on, D. O. S., 234.
- Valves, Dr. M'Dowel's case of disease of the aortic, D. P. S., 229.
- Veins, urate of lime in the coats of the, Dr. Schroeder van der Kolk on, *Trans.*, 502.
- Venereal affections, Dr. Pietro Gamberini on the truces and cure of, *Trans.*, 476.
- Vertebræ, caries of the cervical, Professor Smith on, D. P. S., 438.
- Virchow, Rud., on tone and atony, *Trans.*, 248.
- Wells, Mr. T. Spencer, on gout and its complications, *Rev.*, 417.
- Wittstein, Dr. G. C., practical chemistry by, *Notice*, v.
- Wöhler, Professor, hand-book of inorganic analysis by, *Notice*, vii.
- Wrist joint, Professor Smith on chronic rheumatic arthritis of the, D. P. S., 223.
- Young, Mr., on a new surgical bed, 111.
- Zenker, Professor, on sarcinæ in the lungs, *Trans.*, 249.